PHILOSOPHICAL

TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXXXVIII.

PART I.

LONDON:

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MDCCCXXXVIII.

ADVERTISEMENT.

The Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions,* take this opportunity to acquaint the Public, that it fully appears, as well from the Council-books and Journals of the Society, as from repeated declarations which have been made in several former *Transactions,* that the printing of them was always, from time to time, the single act of the respective Secretaries till the Forty-seventh Volume; the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgement of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body,

upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

A List of Public Institutions and Individuals, entitled to receive a copy of the Philosophical Transactions of each year, on making application for the same directly or through their respective agents, within five years of the date of publication.

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The Royal Academy of Sciences at Stockholm.
The Royal Society of Sciences at Drontheim.

United States.

The American Philosophical Society at Philadelphia.
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The University of Edinburgh.
The University of Glasgow.
The Observatory at Oxford.
The Observatory at Cambridge.
The Observatory at Dublin.
The Observatory at Armagh.
The Observatory at the Cape of Good Hope.
The Observatory at Paramatta.
The Observatory at Madras.
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The Royal Society, Edinburgh.
The Astronomical Institution, Edinburgh.
The President of the Royal Society.
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The Plumian Professor of Astronomy, Cambridge.
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Dr. William Olbers, of Bremen.
The Dépôt de la Marine, Paris.
The Bowden College, United States.
The Waterville College, United States.

ROYAL MEDALS.

HER MAJESTY QUEEN VICTORIA, in restoring the Foundation of the Royal Medals, has been graciously pleased to approve of the following regulations for the award of them:

That the Royal Medals be given for such papers only as have been presented to the Royal Society, and inserted in their Transactions.

That the triennial Cycle of subjects be the same as that hitherto in operation: viz.

1. Astronomy; Physiology, including the Natural History of Organized Beings.
2. Physics; Geology and Mineralogy.
3. Mathematics; Chemistry.

That, in case no paper, coming within these stipulations, should be considered deserving of the Royal Medal, in any given year, the Council have the power of awarding such Medal to the author of any other paper on either of the several subjects forming the Cycle, that may have been presented to the Society and inserted in their Transactions; preference being given to the subjects of the year immediately preceding: the award being, in such case, subject to the approbation of Her Majesty.

The Council propose to give one of the Royal Medals in the present year (1838) for the most important unpublished paper in Mathematics, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1835, and prior to the termination of the present Session (June 1838).

The Council propose also to give one of the Royal Medals in the present year (1838)

for the most important unpublished paper in Chemistry, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1835, and prior to the termination of the present Session (June 1838).

The Council propose to give one of the Royal Medals in the year 1839 for the most important unpublished paper in Astronomy, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1836, and prior to the termination of the Session in June 1839.

The Council propose to give also one of the Royal Medals in the year 1839 for the most important unpublished paper in Physiology, including the Natural History of Organized Beings, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1836, and prior to the termination of the Session in June 1839.

The Council propose to give one of the Royal Medals in the year 1840 for the most important unpublished paper in Physics, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1837, and prior to the termination of the Session in June 1840.

The Council propose also to give one of the Royal Medals in the year 1840 for the most important unpublished paper in Geology or Mineralogy, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1837, and prior to the termination of the Session in June 1840.

The Council propose to give one of the Royal Medals in the year 1841 for the most important unpublished paper in Mathematics, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1838, and prior to the termination of the Session in June 1841.

The Council propose also to give one of the Royal Medals in the year 1841 for the most important unpublished paper in Chemistry, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1838, and prior to the termination of the Session in June 1841.

Adjudication of the Medals of the Royal Society for the year 1837 by His Royal Highness the President and Council.

A Copley Medal to M. Becquerel, for his various Memoirs on the subject of Electricity, published in the "Mémoires de l'Académie Royale des Sciences de l'Institut de France," and particularly those on the Production of Crystals of Metallic Sulphurets and Sulphur, by the long-continued action of Electricity of very low tension, and published in the tenth volume of those "Mémoires."

Another Copley Medal to John Frederick Daniell, Esq., F.R.S., for his two papers "On Voltaic Combinations," published in the Philosophical Transactions for 1836.

The Royal Medal, in the department of Physics, to the Rev. William Whewell, M.A., F.R.S., for his "Researches connected with the Theory of the Tides," communicated to the Royal Society, and published in its Transactions within the three preceding years.

The Royal Medal, in the department of Geology, for the year 1837, was not awarded.

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PRESENTS

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THE ROYAL SOCIETY,

WITH THE

NAMES OF THE DONORS.

From 17th November 1831, to 21st June 1832.

PRESENTS.	DONORS.
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——— Académie des Sciences et Belles Lettres. Journal des Séances depuis le 3 Octob. 1829 jusqu'au 2 Avril 1831. 4to.	The Academy.
Cambridge Philosophical Society. Transactions, Vol. IV. Part I. 4to. *Cambridge* 1832.	The Society.
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Edinburgh, Royal Society of. Transactions, Vol. XI. Part II. *Edin.* 1831.	The Society.
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London:—	
Asiatic Society (Royal). Proceedings, Feb. 4 to May 19, 1832. 8vo. *Lond.* 1832.	The Society.
Astronomical Society of London. Memoirs, Vol. IV. Part II. 4to. *Lond.* 1831.	The Society.
——— Monthly Notices of Proceedings. 8vo. *Lond.* 1831.	Messrs. Priestley and Weale.
British Association for the Advancement of Science. Report of the First and Second Meetings of the Association, at York in 1831, and at Oxford in 1832. 8vo. *Lond.* 1833.	The Committee.
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Manchester Literary and Philosophical Society. Memoirs, Vol. V. New Series. 8vo. *Manchester* 1831.	The Society.
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Yorkshire Philosophical Society. Annual Report for 1831. 8vo. *York* 1831.	The Society.
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Journal of the Franklin Institute of Pennsylvania; edited by T. P. Jones. New Series. Vols. III.—VII. Nos. 1—8. 8vo. *Philadelphia* 1829–31.	The Franklin Institute.
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PHILOSOPHICAL TRANSACTIONS.

I. *Experimental Researches in Electricity.—Eleventh Series.* By MICHAEL FARADAY, *Esq., D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c. &c.*

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§. 18. *On Induction.* ¶ i. *Induction an action of contiguous particles.* ¶ ii. *Absolute charge of matter.* ¶ iii. *Electrometer and inductive apparatus employed.* ¶ iv. *Induction in curved lines.* ¶ v. *Specific inductive capacity.* ¶ vi. *General results as to induction.*

¶ i. *Induction an action of contiguous particles.*

1161. THE science of electricity is in that state in which every part of it requires experimental investigation; not merely for the discovery of new effects, but, what is just now of far more importance, the development of the means by which the old effects are produced, and the consequent more accurate determination of the first principles of action of the most extraordinary and universal power in nature:—and to those philosophers who pursue the inquiry zealously yet cautiously, combining experiment with analogy, suspicious of their preconceived notions, paying more respect to a fact than a theory, not too hasty to generalize, and above all things, willing at every step to cross-examine their own opinions, both by reasoning and experiment, no branch of knowledge can afford so fine and ready a field for discovery as this. Such is most abundantly shown to be the case by the progress which electricity has made in the last thirty years: Chemistry and Magnetism have successively acknowledged its over-ruling influence; and it is probable that every effect depending upon the powers of inorganic matter, and perhaps most of those related to vegetable and animal life, will ultimately be found subordinate to it.

1162. Amongst the actions of different kinds into which electricity has conventionally been subdivided, there is, I think, none which excels, or even equals in importance that called *Induction*. It is of the most general influence in electrical phenomena, appearing to be concerned in every one of them, and has in reality the

character of a first, essential, and fundamental principle. Its comprehension is so important, that I think we cannot proceed much further in the investigation of the laws of electricity without a more thorough understanding of its nature; how otherwise can we hope to comprehend the harmony and even unity of action which doubtless governs electrical excitement by friction, by chemical means, by heat, by magnetic influence, by evaporation, and even by the living being?

1163. In the long-continued course of experimental inquiry in which I have been engaged, this general result has pressed upon me constantly, namely, the necessity of admitting two forces, or two forms or directions of a force (516. 517.), combined with the impossibility of separating these two forces (or electricities) from each other, either in the phenomena of statical electricity or those of the current. In association with this, the impossibility under any circumstances, as yet, of absolutely charging matter of any kind with one or the other electricity dwelt on my mind, and made me wish and search for a clearer view than any that I was acquainted with, of the way in which electrical powers and the particles of matter are related; especially in inductive actions, upon which almost all others appeared to rest.

1164. When I discovered the general fact that electrolytes refused to yield their elements to a current when in the solid state, though they gave them forth freely if in the liquid condition (380. 394. 402.), I thought I saw an opening to the elucidation of inductive action, and the possible subjugation of many dissimilar phenomena to one law. For let the electrolyte be water, a plate of ice being coated with platina foil on its two surfaces, and these coatings connected with any continued source of the two electrical powers, the ice will charge like a Leyden arrangement, presenting a case of common induction, but no current will pass. If the ice be liquified, the induction will fall to a certain degree, because a current can now pass; but its passing is dependent upon a *peculiar molecular arrangement* of the particles consistent with the transfer of the elements of the electrolyte in opposite directions, the degree of discharge and the quantity of elements evolved being exactly proportioned to each other (377. 783.). Whether the charging of the metallic coating be effected by a powerful electrical machine, a strong and large voltaic battery, or a single pair of plates, makes no difference in the principle, but only in the degree of action (360.). Common induction takes place in each case if the electrolyte be solid, or if fluid chemical action and decomposition ensue, provided opposing actions do not interfere; and it is of high importance occasionally thus to compare effects in their extreme degrees, for the purpose of enabling us to comprehend the nature of an action in its weak state, which may be only sufficiently evident to us in its stronger condition. As, therefore, in the electrolyte, *induction* appeared to be the *first* step, and *decomposition* the *second* (the power of separating these steps from each other by giving the solid or fluid condition being in our hands); as the induction was the same in its nature as that through air, glass, wax, &c. produced by any of the ordinary means; and as the whole effect in the electrolyte appeared to be an action of the particles thrown into

a peculiar or polarized state, I was led to suspect that common induction itself was in all cases an *action of contiguous particles*, and that electrical action at a distance (i. e. ordinary inductive action) never occurred except through the intermediate influence of the intervening matter.

1165. The respect which I entertain towards the names of Epinus, Cavendish, Poisson, and other most eminent men, all of whose theories I believe consider induction as an action at a distance and in straight lines, long indisposed me to the view I have just stated; and though I always watched for opportunities to prove the opposite opinion, and made such experiments occasionally as seemed to bear directly on the point, as, for instance, the examination of electrolytes, solid and fluid, whilst under induction by polarized light (951. 955.), it is only of late, and by degrees, that the extreme generality of the subject has urged me still further to extend my experiments and publish my view. At present I believe ordinary induction in all cases to be an action of contiguous particles, consisting in a species of polarity, instead of being an action of either particles or masses at sensible distances; and if this be true, the distinction and establishment of such a truth must be of the greatest consequence to our further progress in the investigation of the nature of electric forces. The linked condition of electrical induction with chemical decomposition; of voltaic excitement with chemical action; the transfer of elements in an electrolyte; the original cause of excitement in all cases; the nature and relation of conduction and insulation; of the direct and lateral or transverse action constituting electricity and magnetism; with many other things more or less incomprehensible at present, would all be affected by it, and perhaps receive a full explication in their reduction under one general law.

1166. I searched for an unexceptionable test of my view, not merely in the accordance of known facts with it, but in the consequences which would flow from it if true; especially in those which would not be consistent with the theory of action at a distance. Such a consequence seemed to me to present itself in the direction in which inductive action could be exerted. If in straight lines only, though not perhaps decisive, it would be against my view; if in curved lines also, that would be a natural result of the action of contiguous particles, but I think utterly incompatible with action at a distance, as assumed by the received theories, which, according to every fact and analogy we are acquainted with, is always in straight lines.

1167. Again, if induction be an action of contiguous particles, and also the first step in the process of electrolyzation (1164. 949.), there seemed reason to expect some particular relation of it to the different kinds of matter through which it would be exerted, or something equivalent to a specific electric induction for different bodies, which, if it existed, would unequivocally prove the dependence of induction on the particles; and though this, in the theory of Poisson and others, has never been supposed to be the case, I was soon led to doubt the received opinion, and have taken great pains in subjecting this matter to close experimental examination.

1168. Another ever present question on my mind has been, whether electricity has

an actual and independent existence as a fluid or fluids, or was a mere power of matter, like what we conceive of the attraction of gravitation. If determined either way it would be an enormous advance in our knowledge; and as having the most direct and influential bearing on my notions, I have always sought for experiments which would in any way tend to elucidate that great question. It was in attempts to prove the existence of electricity separate from matter, by giving an independent charge of either positive or negative power to some substance, and the utter failure of all such attempts, whatever substance was used or whatever means of exciting or *evolving* electricity were employed, that first drove me to look upon induction as an action of the particles of matter, each having *both* forces developed in it in exactly equal amount. It is this circumstance, in connection with others, which makes me desirous of placing the remarks on absolute charge first, in the order of proof and argument, which I am about to adduce in favour of my view, that electric induction is an action of the contiguous particles of the insulating medium or *di-electric*.

¶ ii. *On the absolute charge of matter.*

1169. Can matter, either conducting or non-conducting, be charged with one electric force independently of the other, in the least degree, either in a sensible or latent state?

1170. The beautiful experiments of Coulomb upon the equality of action of *conductors*, whatever their substance, and the residence of *all* the electricity upon their surfaces*, are sufficient, if properly viewed, to prove that *conductors cannot be bodily charged*; and as yet no means of communicating electricity to a conductor so as to relate its particles to one electricity, and not at the same time to the other in exactly equal amount, has been discovered.

1171. With regard to electrics or non-conductors, the conclusion does not at first seem so clear. They may easily be electrified bodily, either by communication (1247.) or excitement; but being so charged, every case in succession, when examined, came out to be a case of induction, and not of absolute charge. Thus, glass within conductors could easily have parts not in contact with the conductor brought into an excited state; but it was always found that a portion of the inner surface of the conductor was in an opposite and equivalent state, or that another part of the glass itself was in an equally opposite state, an *inductive* charge and not an *absolute* charge having been acquired.

1172. Well-purified oil of turpentine, which I find to be an excellent liquid insulator for most purposes, was put into a metallic vessel, and, being insulated, was charged, sometimes by contact of the metal with the electrical machine, and at others by a wire dipping into the fluid within; but whatever the mode of communication, no electricity of one kind was retained by the arrangement, except what appeared on the exterior surface of the metal, that portion being there only by an inductive action through the air around. When the oil of turpentine was confined in glass

* Memoires de l'Academie, 1786, pp. 67. 69. 72; 1787, p. 452.

vessels, there were at first some appearances as if the fluid did receive an absolute charge of electricity from the charging wire, but these were quickly reduced to cases of common induction jointly through the fluid, the glass, and the surrounding air.

1173. I carried these experiments on with air to a very great extent. I had a chamber built, being a cube of twelve feet in the side. A slight cubical wooden frame was constructed, and copper wire passed along and across it in various directions, so as to make the sides a large net-work, and then all was covered in with paper, placed in close connection with the wires, and supplied in every direction with bands of tin foil, that the whole might be brought into good metallic communication, and rendered a free conductor in every part. This chamber was insulated in the lecture-room of the Royal Institution; a glass tube above six feet in length was passed through its side, leaving about four feet within and two feet on the outside, and through this a wire passed from the large electrical machine (290.) to the air within. By working the machine, the air within this chamber could be brought into what is considered a highly electrified state (being, in fact, the same state as that of the air of a room in which a powerful machine is in operation), and at the same time the outside of the insulated cube was everywhere strongly charged. But putting the chamber in communication with the perfect discharging train described in a former series (292.), and working the machine so as to bring the air within to its utmost degree of charge, if I quickly cut off the connexion with the machine, and at the same moment or instantly after insulated the cube, the air within had not the least power to communicate a further charge to it. If any portion of the air was electrified, as glass or other insulators may be charged (1171.), it was accompanied by a corresponding opposite action *within* the cube, the whole effect being merely a case of induction. Every attempt to charge air bodily and independently with the least portion of either electricity failed.

1174. I put a delicate gold-leaf electrometer within the cube, and then charged the whole by an *outside* communication, very strongly, for some time together; but neither during the charge or after the discharge did the electrometer or air within show the least signs of electricity. I charged and discharged the whole arrangement in various ways, but in no case could I obtain the least indication of an absolute charge; or of one by induction in which the electricity of one kind had the smallest superiority in quantity over the other. I went into the cube and lived in it, and using lighted candles, electrometers, and all other tests of electrical states, I could not find the least influence upon them, or indication of anything particular given by them, though all the time the outside of the cube was powerfully charged, and large sparks and brushes were darting off from every part of its outer surface. The conclusion I have come to is, that non-conductors, as well as conductors, have never yet had an absolute and independent charge of one electricity communicated to them, and that to all appearance such a state of matter is impossible.

1175. There is another view of this question which may be taken under the sup-

position of the existence of an electric fluid or fluids. It may be impossible to have the one fluid or state in a free condition without its producing by induction the other, and yet possible to have cases in which an isolated portion of matter in one condition being uncharged, shall, by a change of state, evolve one electricity or the other: and though such evolved electricity might immediately induce the opposite state in its neighbourhood, yet the mere evolution of one electricity without the other in the *first instance*, would be a very important fact in the theory which assumes a fluid or fluids: these theories as I understand them assigning not the slightest reason why such an effect should not occur.

1176. But on searching for such cases I cannot find one. Evolution by friction, as is well known, gives both powers in equal proportion. So does evolution by chemical action, notwithstanding the great diversity of bodies which may be employed, and the enormous quantity of electricity which can in this manner be evolved (371. 376. 861. 868.) The more promising cases of change of state, whether by evaporation, fusion, or the reverse processes, still give both forms of the power in *equal* proportion; and the cases of splitting of mica and other crystals, the breaking of sulphur, &c. &c., are subject to the same limitation.

1177. As far as experiment has proceeded, it appears, therefore, impossible either to evolve or make disappear one electric force without equal and corresponding change in the other. It is also equally impossible experimentally to charge a portion of matter with one electric force independently of the other. Charge always implies *induction*, for it can in no instance be effected without; and also the presence of the *two* forms of power, equally at the moment of development and afterwards. There is no *absolute* charge of matter with one fluid; no latency of a single electricity. This though a negative result is an exceedingly important one, being probably the consequence of a natural impossibility, which will become clear to us when we understand the true condition and theory of the electric power.

1178. The preceding considerations already point to the following conclusions: bodies cannot be charged absolutely, but only relatively, and by a principle which is the same with that of *induction*. All *charge* is sustained by induction. All phenomena of *intensity* include the principle of induction. All *excitation* is dependent on or directly related to induction. All *currents* involve previous intensity and therefore previous induction. INDUCTION appears to be the essential function both in the first development and the consequent phenomena of electricity.

¶ iii. *Electrometer and inductive apparatus employed.*

1179. Leaving for a time the further consideration of the preceding facts until they can be collated with other results bearing directly on the great question of the nature of induction, I will now describe the apparatus I have had occasion to use; and in proportion to the importance of the principles sought to be established is the necessity of doing this so clearly, as to leave no doubt of the results behind.

1180. *Electrometer.* The measuring instrument I have employed has been the torsion balance electrometer of COULOMB, constructed, generally, according to his instructions*, but with certain variations and additions, which I will briefly describe. The lower part was a glass cylinder eight inches in height and eight inches in diameter; the tube for the torsion thread was seventeen inches in length. The torsion thread itself was not of metal, but glass, according to the excellent suggestion of the late Dr. RITCHIE†. It was twenty inches in length, and of such tenuity that when the shell lac lever and attached ball, &c. were connected with it, they made about ten vibrations in a minute. It would bear torsion through four revolutions or 1440°, and yet, when released, return accurately to its position; probably it would have borne considerably more than this without injury. The repelled ball was of pith, gilt, and was 0·3 of an inch in diameter. The horizontal stem or lever supporting it was of shell lac, according to COULOMB's direction, the arm carrying the ball being 2·4 inches long, and the other only 1·2 inches: to this was attached the vane, also described by COULOMB, which I found to answer admirably its purpose of quickly destroying vibrations. That the inductive action within the electrometer might be uniform in all positions of the repelled ball and in all states of the apparatus, two bands of tin foil, about an inch wide each, were attached to the inner surface of the glass cylinder, going entirely round it, at the distance of 0·4 of an inch from each other, and at such a height that the intermediate clear surface was in the same horizontal plane with the lever and ball. These bands were connected with each other and with the earth, and, being perfect conductors, always exerted a uniform influence on the electrified balls within, which the glass surface, from its irregularity of condition at different times, I found, did not. For the purpose of keeping the air within the electrometer in a constant state as to dryness, a glass dish, of such size as to enter easily within the cylinder, had a layer of fused potash placed within it, and this being covered with a disc of fine wire gauze to render its inductive action uniform at all parts, was placed within the instrument at the bottom and left there.

1181. The moveable ball used to take and measure the portion of electricity under examination, and which may be called the *repelling*, or the *carrier*, ball, was of soft alder wood, well and smoothly gilt. It was attached to a fine shell lac stem, and introduced through a hole into the electrometer according to COULOMB's method: the stem was fixed at its upper end in a block or vice, supported on three short feet; and on the surface of the glass cover above was a plate of lead with stops on it, so that when the carrier ball was adjusted in its right position, with the vice above bearing at the same time against these stops, it was perfectly easy to bring away the carrier ball and restore it to its place again very accurately, without any loss of time.

1182. It is quite necessary to attend to certain precautions respecting these balls. If of pith alone they are bad; for when very dry, that substance is so imperfect a

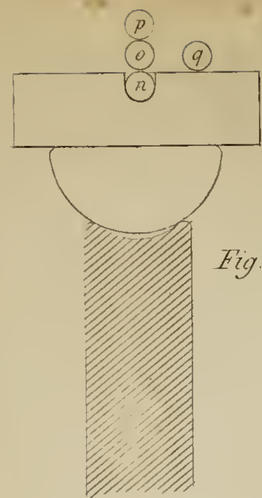
* Memoires de l'Academie, 1785, p. 570.

† Philosophical Transactions, 1830.

conductor that it neither receives nor gives a charge freely, and so, after contact with a charged conductor, is liable to be in an uncertain condition. Again, it is difficult to turn pith so smoothly as to leave the ball, even when gilt, sufficiently free from irregularities of form, as to retain its charge undiminished for a considerable length of time. When, therefore, the balls are finally prepared and gilt they should be examined, and being electrified, unless they can hold their charge with very little diminution for a considerable time, and yet be discharged instantly and perfectly by the touch of an uninsulated conductor, they should be dismissed.

1183. It is, perhaps, unnecessary to refer to the graduation of the instrument, further than to explain how the observations were made. On a circle or ring of paper on the outside of the glass cylinder, fixed so as to cover the internal lower ring of tin foil, were marked four points corresponding to angles of 90°; four other points exactly corresponding to these points being marked on the upper ring of tin foil within. By these and the adjusting screws on which the whole instrument stands, the glass torsion thread could be brought accurately into the centre of the instrument and of the graduations on it. From one of the four points on the exterior of the cylinder a graduation of 90° was set off, and a corresponding graduation was placed upon the upper tin foil on the opposite side of the cylinder within; and a dot being marked on that point of the surface of the repelled ball nearest to the side of the electrometer, it was easy, by observing the line which this dot made with the lines of the two graduations just referred to, to ascertain accurately the position of the ball. The upper end of the glass thread was attached, as in COULOMB's original electrometer, to an index, which had its appropriate graduated circle, upon which the degree of torsion was ultimately to be read off.

1184. After the levelling of the instrument and adjustment of the glass thread, the blocks which determine the place of the *carrier ball* are to be regulated (1181.) so that, when the carrier arrangement is placed against them, the centre of the ball may be in the radius of the instrument corresponding to 0° on the lower graduation or that on the side of the electrometer, and at the same level and distance from the centre as the *repelled ball* on the suspended torsion lever. Then the torsion index is to be turned until the ball connected with it (the repelled ball) is accurately at 30°, and finally the graduated arch belonging to the torsion index is to be adjusted so as to bring 0° upon it to the index. This state of the instrument was adopted as that which gave the most direct expression of the experimental results, and in the form having fewest variable errors; the angular distance of 30° being always retained as the standard distance to which the balls were in every case to be brought, and the whole of the torsion being read off at once on the graduated circle above. Under these circumstances the distance of the balls from each other was not merely the same in degree, but their position in the instrument, and in relation to every part of it, was actually the same every time that a measurement was made; so that all irregularities arising from slight difference of form and action in the instrument and the



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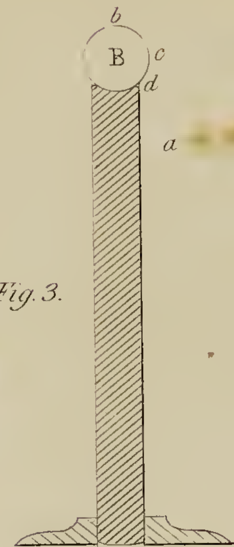

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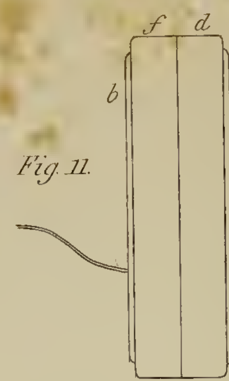

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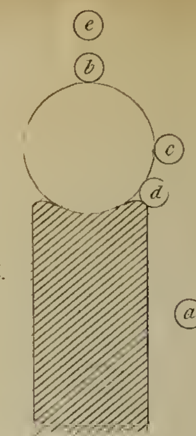

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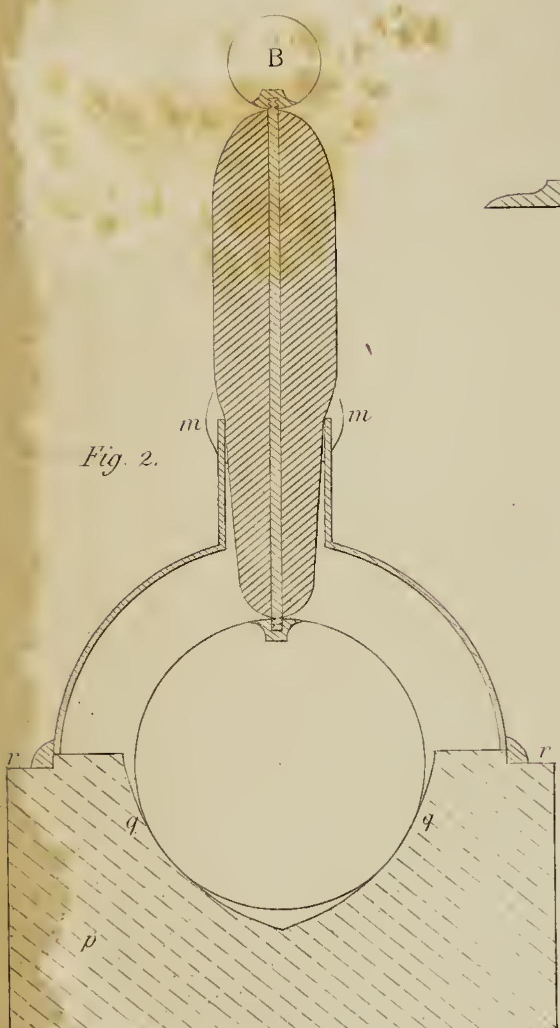

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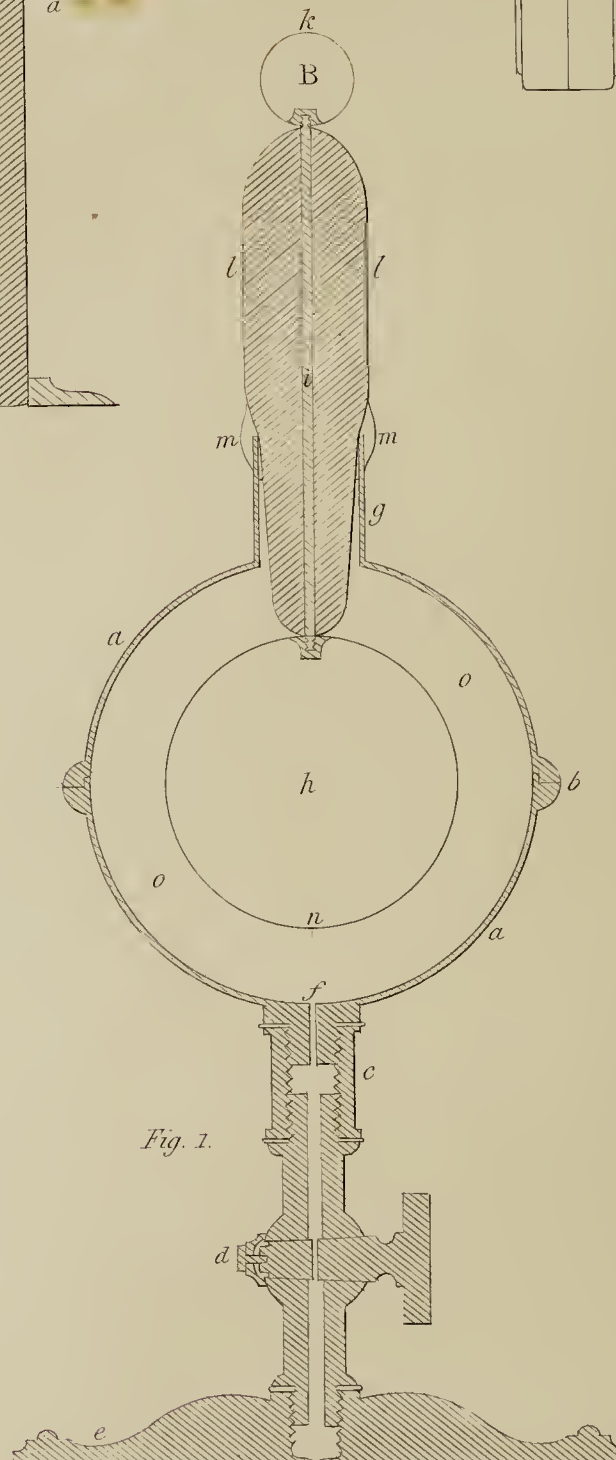

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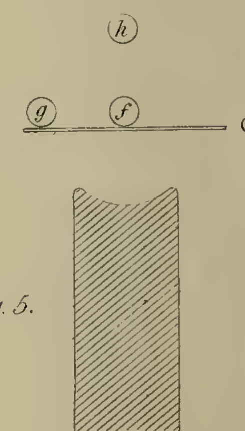

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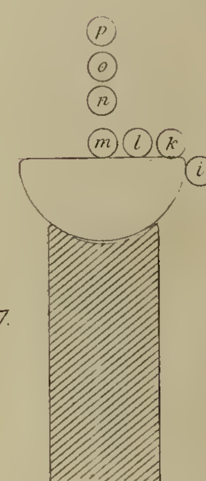

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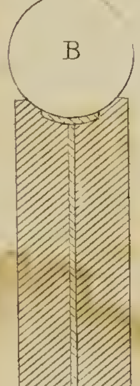

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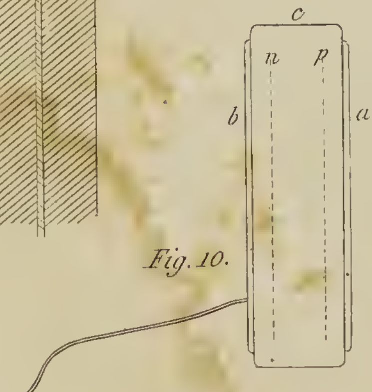


Fig. 10.

Fig. 8.

bodies around were avoided. The only difference which could occur in the position of anything within, consisted in the deflexion of the torsion thread from a vertical position, more or less, according to the force of repulsion of the balls; but this was so slight as to cause no interfering difference in the symmetry of form within the instrument, and gave no error in the amount of torsion force indicated on the graduation above.

1185. Although the constant angular distance of 30° between the centres of the balls was adopted, and found abundantly sensible, for all ordinary purposes, yet the facility of rendering the instrument far more sensible by diminishing this distance was at perfect command; the results at different distances being very easily compared with each other either by experiment, or, as they are inversely as the squares of the distances, by calculation.

1186. The Coulomb balance electrometer requires experience to be understood; but I think it a very valuable instrument in the hands of those who will take pains by practice and attention to learn the precautions needful in its use. Its insulating condition varies with circumstances, and should be examined before it is employed in experiments. In an ordinary and fair condition, when the balls were so electrified as to give a repulsive torsion force of 400° at the standard distance of 30°, it took nearly four hours to sink to 50° at the same distance; the average loss from 400° to 300° being at the rate of 2°·7 per minute, from 300° to 200° of 1°·7 per minute, from 200° to 100° of 1°·3 per minute, and from 100° to 50° of 0°·87 per minute. As a complete measurement by the instrument may be made in much less than a minute, the amount of loss in that time is but small, and can easily be taken into account.

1187. *The inductive apparatus.*—My object was to examine inductive action carefully when taking place through different media, for which purpose it was necessary to subject these media to it in exactly similar circumstances, and in such quantities as should suffice to eliminate any variations they might present. The requisites of the apparatus to be constructed were, therefore, that the inducing surfaces of the conductors should have a constant form and state, and be at a constant distance from each other; and that either solids, fluids, or gases might be placed and retained between these surfaces with readiness and certainty, and for any length of time.

1188. The apparatus used may be described in general terms as consisting of two metallic spheres of unequal diameter, placed, the smaller within the larger, and concentric with it; the interval between the two being the space through which the induction was to take place. A section of it is given (Plate I. fig. 1.) on a scale of one half: *a*, *a* are the two halves of a brass sphere, with an air-tight joint at *b*, like that of the Magdeburg hemispheres, made perfectly flush and smooth inside so as to present no irregularity; *c* is a connecting piece by which the apparatus is joined to a good stop-cock *d*, which is itself attached either to the metallic foot *e*, or to an air pump. The aperture within the hemisphere at *f* is very small: *g* is a brass collar fitted to the upper hemisphere, through which the shell lac support of the inner ball

and its stem passes; *h* is the inner ball, also of brass; it screws on to a brass stem *i*, terminated above by a brass ball B; *l*, *l* is a mass of shell lac, moulded carefully on to *i*, and serving both to support and insulate it and its balls *h*, B. The shell-lac stem *l* is fitted into the socket *g*, by a little ordinary resinous cement, more fusible than shell-lac, applied at *m m* in such a way as to give sufficient strength and render the apparatus air-tight there, yet leave as much as possible of the lower part of the shell-lac stem untouched, as an insulation between the ball *h* and the surrounding sphere *a*, *a*. The ball *h* has a small aperture at *n*, so that when the apparatus is exhausted of one gas and filled with another, the ball *h* may itself also be exhausted and filled, that no variation of the gas in the interval *o* may occur during the course of an experiment.

1189. It will be unnecessary to give the dimensions of all the parts, since the drawing is to a scale of one half: the inner ball has a diameter of 2·33 inches, and the surrounding sphere an internal diameter of 3·57 inches. Hence the width of the intervening space, through which the induction is to take place, is 0·62 of an inch; and the extent of this place or plate, i. e. the surface of a medium sphere, may be taken as twenty-seven square inches, a quantity considered as sufficiently large for the comparison of different substances. Great care was taken in finishing well the inducing surfaces of the ball *h* and sphere *a*, *a*; and no varnish or lacquer was applied to them, or to any part of the metal of the apparatus.

1190. The attachment and adjustment of the shell-lac stem was a matter requiring considerable care, especially as, in consequence of its cracking, it had frequently to be renewed. The best lac was chosen and applied to the wire *i*, so as to be in good contact with it everywhere, and in perfect continuity throughout its own mass. It was not thinner than is given by scale in the drawing, for when less it frequently cracked within a few hours after its cooling. I think that very slow cooling or annealing improved its quality in this respect. The collar *g* was made as thin as could be, that the lac might be as large there as possible. In order that at every re-attachment of the stem to the upper hemisphere the ball *h* might have the same relative position, a gauge *p* (fig. 2.) was made of wood, and this being applied to the ball and hemisphere whilst the cement at *m* was still soft, the bearings of the ball at *q q*, and the hemisphere at *r r*, were forced home, and the whole left until cold. Thus all difficulty in the adjustment of the ball in the sphere was avoided.

1191. I had occasion at first to attach the stem to the socket by other means, as a band of paper or a plugging of white silk thread; but these were very inferior to the cement, interfering much with the insulating power of the apparatus.

1192. The retentive power of this apparatus was, when in good condition, better than that of the electrometer (1186.), i. e. the proportion of loss of power was less. Thus when the apparatus was electrified, and also the balls in the electrometer, to such a degree, that after the inner ball had been in contact with the top *k* of the ball of the apparatus, it caused a repulsion indicated by 600° of torsion force, then

in falling from 600° to 400° the average loss was 8°·6 per minute; from 400° to 300° the average loss was 2°·6 per minute; from 300° to 200° it was 1°·7 per minute; from 200° to 170° it was 1° per minute. This was after the apparatus had been charged for a short time; at the first instant of charging there is an apparent loss of electricity, which can only be comprehended hereafter (1207. 1250.).

1193. When the apparatus loses its insulating power suddenly, it is almost always from a crack near to or within the brass socket. These cracks are usually transverse to the stem. If they occur at the part attached by common cement to the socket, the air cannot enter, and being then as vacua, they conduct away the electricity and lower the charge, as fast almost as if a piece of metal had been introduced there. Occasionally stems in this state, being taken out and cleared from the common cement, may, by the careful application of the heat of a spirit lamp, be so far softened and melted as to renew perfect continuity of the parts; but if that does not succeed in restoring things to a good condition, the remedy is a new shell-lac stem.

1194. The apparatus when in order could easily be exhausted of air and filled with any given gas; but when that gas was acid or alkaline, it could not properly be removed by the air-pump, and yet required to be perfectly cleared away. In such cases the apparatus was opened and cleared; and with respect to the inner ball *h*, it was washed out two or three times with distilled water introduced at the screw hole, and then being heated above 212°, air was blown through to render the interior perfectly dry.

1195. The inductive apparatus described is evidently a Leyden phial, with the advantage, however, of having the dielectric or insulating medium changed at pleasure. The balls *h* and B, with the connecting wire *i*, constitute the charged conductor, upon the surface of which all the electric force is resident by virtue of induction (1178.). Now though the largest portion of this induction is between the ball *h* and the surrounding sphere *a a*, yet the wire *i* and the ball B determine a part of the induction from their surfaces towards the external surrounding conductors. Still, as all things in that respect remain the same, whilst the medium within at *o o*, may be varied, any changes exhibited by the whole apparatus will in such cases depend upon the variations made in the interior; and it was these changes I was in search of, the negation or establishment of such differences being the great object of my inquiry. I considered that these differences, if they existed, would be most distinctly set forth by having two apparatus of the kind described, precisely similar in every respect; and then, different insulating media being within, to charge one and measure it, and after dividing the charge with the other, to observe what the ultimate conditions of both were. If insulating media really had any specific differences in favouring or opposing inductive action through them, such differences, I conceived, could not fail of being developed by such a process.

1196. I will wind up this description of the apparatus, and explain the precautions necessary in their use, by describing the form and order of the experiments made to

prove their equality when both contained common air. In order to facilitate reference I will distinguish the two by the terms App. i. and App. ii.

1197. The electrometer is first to be adjusted and examined (1184.), and the app. i. and ii. are to be perfectly discharged. A Leyden phial is to be charged to such a degree that it would give a spark of about one-sixteenth or one-twentieth of an inch in length between two balls of half an inch diameter; and the carrier ball of the electrometer being charged by this phial, is to be introduced into the electrometer, and the lever ball brought by the motion of the torsion index against it; the charge is thus divided between the balls, and repulsion ensues. It is useful then to bring the repelled ball to the standard distance of 30° by the motion of the torsion index, and observe the force in degrees required for this purpose; this force will in future experiments be called *repulsion of the balls.*

1198. One of the inductive apparatus, as, for instance, app. i., is now to be charged from the Leyden phial, the latter being in the state it was in when used to charge the balls; the carrier ball is to be brought into contact with the top of its upper ball (*k*, fig. 1.), then introduced into the electrometer, and the repulsive force (at the distance of 30°) measured. Again, the carrier should be applied to the app. i. and the measurement repeated; the apparatus i. and ii. are then to be joined, so as to *divide* the charge, and afterwards the force of each measured by the carrier ball, applied as before, and the results carefully noted. After this both i. and ii. are to be discharged; then app. ii. charged, measured, divided with app. i., and the force of each again measured and noted. If in each case the half charges of app. i. and ii. are equal, and are together equal to the whole charge before division, then it may be considered as proved that the two apparatus are precisely equal in power, and fit to be used in cases of comparison between different insulating media or *dielectrics.*

1199. But the *precautions* necessary to obtain accurate results are numerous. The apparatus i. and ii. must always be placed on a thoroughly uninsulating medium. A mahogany table, for instance, is far from satisfactory in this respect, and therefore a sheet of tin foil, connected with an extensive discharging train (292.), is what I have used. They must be so placed also as not to be too near each other, and yet equally exposed to the inductive influence of surrounding objects; and these objects, again, should not be disturbed in their position during an experiment, or else variations of induction upon the external ball B of the apparatus may occur, and so errors be introduced into the results. The carrier ball, when receiving its portion of electricity from the apparatus, should always be applied at the same part of the ball, as, for instance, the summit *k*, and always in the same way; variable induction from the vicinity of the head, hands, &c. being avoided, and the ball after contact being withdrawn upwards in a regular and constant manner.

1200. As the stem had occasionally to be changed (1190.), and the change might occasion slight variations in the position of the ball within, I made such a variation

purposely, to the amount of an eighth of an inch (which is far more than ever could occur in practice), but did not find that it sensibly altered the relation of the apparatus, or its inductive condition *as a whole*. Another trial of the apparatus was made as to the effect of dampness in the air, one being filled with very dry air, and the other with air from over water. Though this produced no change in the result, except an occasional tendency to more rapid dissipation, yet the precaution was always taken when working with gases (1290.) to dry them perfectly.

1201. It is essential that the interior of the apparatus should be *perfectly* free from dust or small loose particles, for these very rapidly lower the charge and interfere on occasions when their presence and action would hardly be expected. To breathe on the interior of the apparatus and wipe it out quietly with a clean silk handkerchief, is an effectual way of removing them; but then the intrusion of other particles should be carefully guarded against, and a dusty atmosphere should for this and several other reasons be avoided.

1202. The shell lac stem requires occasionally to be well wiped, to remove, in the first instance, the film of wax and adhering matter which is upon it; and afterwards to displace dirt and dust which will gradually attach to it in the course of experiments. I have found much to depend upon this precaution, and a silk handkerchief is the best wiper.

1203. But wiping and some other circumstances tend to give a charge to the surface of the shell lac stem. This should be removed, for, if allowed to remain, it very seriously affects the degree of charge given to the carrier ball by the apparatus (1232.). This condition of the stem is best observed by discharging the apparatus, applying the carrier ball to the stem, touching it with the finger, insulating and removing it, and examining whether it has received any charge (by induction) from the stem; if it has, the stem itself is in a charged state. The best method of removing the charge I have found to be, to cover the finger with a single fold of a silk handkerchief, and breathing on the stem, to wipe it immediately after with the finger, the ball B and its connected wire, &c. being at the same time *uninsulated*: the wiping place of the silk must not be changed; it then becomes sufficiently damp not to excite the stem, and is yet dry enough to leave it in a clean and excellent insulating condition. If the air be dusty, it will be found that a single charge of the apparatus will bring on an electric state of the outside of the stem, in consequence of the carrying power of the particles of dust; whereas in the morning, and in a room which has been left quiet, several experiments can be made in succession without the stem assuming the least degree of charge.

1204. Experiments should not be made by candle or lamp light except with much care, for flames have great and yet unsteady powers of affecting and dissipating electrical charges.

1205. As a final observation on the state of the apparatus, they should retain their charge well and uniformly, and alike for both, and at the same time allow of a per-

fect and instantaneous discharge, giving then no charge to the carrier ball, whatever part of the ball B it may be applied to (1218.).

1206. With respect to the balance electrometer all the precautions that need be mentioned, are, that the carrier ball is to be preserved during the first part of an experiment in its electrified state, the loss of electricity which would follow upon its discharge being avoided; and, that in introducing it into the electrometer through the hole in the glass plate above, care should be taken that it do not touch, or even come near to, the edge of the glass.

1207. When the whole charge in one apparatus is divided between the two, the gradual fall, apparently from dissipation, in the apparatus which has *received* the half charge is greater than in the one *originally* charged. This is due to a peculiar effect to be described hereafter (1250. 1251.), the interfering influence of which may be avoided to a great extent by going through the steps of the process regularly and quickly; therefore, after the original charge has been measured, in app. i. for instance, i. and ii. are to be symmetrically joined by their balls B, the carrier touching one of these balls at the same time; it is first to be removed, and then the apparatus separated from each other; app. ii. is next quickly to be measured by the carrier, then app. i.; lastly, ii. is to be discharged, and the discharged carrier applied to it to ascertain whether any residual effect is present (1205.), and app. i. being discharged is also to be examined in the same manner and for the same purpose.

1208. The following is an example of the division of a charge by the two apparatus, air being the dielectric in both of them. The observations are set down one under the other in the order in which they were taken, the left hand numbers representing the observations made on app. i. and the right hand numbers those on app. ii. App. i. is that which was originally charged, and after two measurements, the charge was divided with app. ii.

App. i.		App. ii.	
Balls 160°			
	. . . .	0°	
254°	. . . .		
250	. . . .		
divided and instantly taken			
	. . . .	122	
124	. . . .		
1	. . . .		after being discharged.
	. . . .	2	after being discharged.

1209. Without endeavouring to allow for the loss which must have been gradually going on during the time of the experiment, let us observe the results of the numbers as they stand. As 1° remained in app. i. in an undischargeable state, 249° may be taken as the utmost amount of the transferable or divisible charge, the half of which is 124°·5. As app. ii. was free of charge in the first instance, and immediately after

the division was found with 122°, this amount *at least* may be taken as what it had received. On the other hand 124° minus 1°, or 123°, may be taken as the half of the transferable charge retained by app. i. Now these do not differ much from each other, or from 124°·5, the half of the full amount of transferable charge; and when the gradual loss of charge evident in the difference between 254° and 250° of app. i. is also taken into account, there is every reason to admit the result as showing an equal division of charge, *unattended by any disappearance of power* except that due to dissipation.

1210. I will give another result, in which app. ii. was first charged, and where the residual action of that apparatus was greater than in the former case.

App. i.	App. ii.	
	Balls 150°	
. . . .	152°	
. . . .	148	
divided and instantly taken		
70°		
. . . .	78	
. . . .	5	immediately after discharge.
0		immediately after discharge.

1211. The transferable charge being 148°—5°, its half is 71°·5, which is not far removed from 70°, the half charge of i.; or from 73°, the half charge of ii.: these half charges again making up the sum of 143°, or just the amount of the whole transferable charge. Considering the errors of experiment, therefore, these results may again be received as showing that the apparatus were equal in inductive capacity, or in their powers of receiving charges.

1212. The experiments were repeated with charges of negative electricity with the same general results.

1213. That I might be sure of the sensibility and action of the apparatus, I made such a change in one as ought upon principle to increase its inductive force, i. e. I put a metallic lining into the lower hemisphere of app. i., so as to diminish the thickness of the intervening air in that part, from 0·62 to 0·435 of an inch: this lining was carefully shaped and rounded so that it should not present a sudden projection within at its edge, but a gradual transition from the reduced interval in the lower part of the sphere to the larger one in the upper.

1214. This change immediately caused app. i. to produce effects indicating that it had a greater aptness or capacity for induction than app. ii. Thus, when a transferable charge in app. ii. of 469° was divided with app. i., the former retained a charge of 225°, whilst the latter showed one of 227°, i. e. the former had lost 244° in communicating 227° to the latter: on the other hand, when app. i. had a transferable charge in it of 381° divided by contact with app. ii., it lost 181° only, whilst it gave to app. ii. as many as 194°:—the sum of the divided forces being in the first instance

less, and in the second instance *greater* than the original undivided charge. These results are the more striking, as only one half of the interior of app. i. was modified, and they show that the instruments are capable of bringing out differences in inductive force from amongst the errors of experiment, when these differences are much less than that produced by the alteration made in the present instance.

¶ iv. *Induction in curved lines.*

1215. Amongst those results deduced from the molecular view of induction (1166.), which, being of a peculiar nature, are the best tests of the truth or error of the theory, the expected action in curved lines is, I think, the most important at present; for, if shown to take place in an unexceptionable manner, I do not see how the old theory of action at a distance and in straight lines can stand, or how the conclusion that ordinary induction is an action of contiguous particles can be resisted.

1216. There are many forms of old experiments which might be quoted as favourable to, and consistent with the view I have adopted. Such are most cases of electro-chemical decomposition, electrical brushes, auras, sparks, &c.; but as these might be considered equivocal evidence, inasmuch as they include a current and discharge, (though they have long been to me indications of prior molecular action (1230.)) I endeavoured to devise such experiments for first proofs as should not include transfer, but relate altogether to the pure simple inductive action of statical electricity.

1217. It was also of importance to make these experiments in the simplest possible manner, using not more than one insulating medium or dielectric at a time, lest differences of slow conduction should produce effects which might erroneously be supposed to result from induction in curved lines. It will be unnecessary to describe the steps of the investigation minutely; I will at once proceed to the simplest mode of proving the facts, first in air and then in other insulating media.

1218. A cylinder of solid shell-lac, 0·9 of an inch in diameter and seven inches in length, was fixed upright in a wooden foot (fig. 3.): it was made concave or cupped at its upper extremity so that a brass ball or other small arrangement could stand upon it. The upper half of the stem having been excited *negatively* by friction with warm flannel, a brass ball, B, 1 inch in diameter, was placed on the top, and then the whole arrangement examined by the carrier ball and Coulomb's electrometer (1180. &c.). For this purpose the balls of the electrometer were charged *positively* to about 360°, and then the carrier being applied to various parts of the ball B, the two were uninsulated whilst in contact or in position, then insulated*, separated, and the charge of the carrier examined as to its nature and force. Its electricity was always positive, and its force at the different positions *a*, *b*, *c*, *d*, &c. (figs. 3. and 4.) observed in succession, was as follows:

* It can hardly be necessary for me to say here, that whatever general state the carrier ball acquired in any place where it was uninsulated and then insulated, it retained on removal from that place, notwithstanding that it might pass through other places that would have given to it, if uninsulated, a different condition.

at *a*	. . above	1000°
b it was	. . .	149
c	. . . . . .	270
d	. . . . . .	512
b	. . . . . .	130

1219. To comprehend the full force of these results, it must first be understood, that all the charges of the ball B and the carrier are charges by induction, from the action of the excited surface of the shell-lac cylinder; for whatever electricity the ball B received by *communication* from the shell-lac, either in the first instance or afterwards, was removed by the uninsulating contacts, only that due to induction remaining; and this is shown by the charges taken from the ball in this its uninsulated state being always positive, or of the contrary character to the electricity of the shell-lac. In the next place the charges at *a*, *c*, and *d* were of such a nature as might be expected from an inductive action in straight lines, but that obtained at *b* is *not so*: it is clearly a charge by induction, but *induction* in *a curved line*; for the carrier ball whilst applied to *b*, and after its removal to a distance of six inches or more from B, could not, in consequence of the size of B, be connected by a straight line with any part of the excited and inducing shell-lac.

1220. To suppose that the upper part of the *uninsulated* ball B, should in some way be retained in an electrified state by that portion of the surface which is in sight of the shell-lac, would be in opposition to what we know already of the subject. Electricity is retained upon the surface of conductors only by induction (1178.); and though some persons may not be prepared as yet to admit this with respect to insulated conductors, all will as regards uninsulated conductors like the ball B; and to decide the matter we have only to place the carrier ball at *e* (fig. 4.), so that it shall not come in contact with B, uninsulate it by a metallic rod descending perpendicularly, insulate it, remove it, and examine its state; it will be found charged with the same kind of electricity as, and even to a higher degree (1224.) than, if it had been in contact with the summit of B.

1221. To suppose, again, that induction acts in some way *through or across* the metal of the ball, is negatived by the simplest considerations; but a fact in proof will be better. If instead of the ball B a small disc of metal be used, the carrier may be charged at, or above the middle of its upper surface: but if the plate be enlarged to about $1\frac{1}{2}$ or 2 inches in diameter, C (fig. 5.), then no charge will be given to the carrier at *f*, though when applied nearer to the edge at *g*, or even *above the middle* at *h*, a charge will be obtained; and this is true though the plate may be a mere thin film of gold leaf. Hence it is clear that the induction is not *through* the metal, but through the air or dielectric, and that in curved lines.

1222. I had another arrangement, in which a wire passing downwards through the middle of the shell-lac cylinder to the earth, was connected with the ball B (fig. 6.)

so as to keep it in a constantly uninsulated state. This was a very convenient form of apparatus, and the results with it were the same as those described.

1223. In another case the ball B was supported by a shell-lac stem, independently of the excited cylinder of shell-lac, and at half an inch distance from it; but the effects were the same. Then the brass ball of a charged Leyden jar was used in place of the excited shell-lac to produce induction; but this caused no alteration of the phenomena. Both positive and negative inducing charges were tried with the same general results. Finally, the arrangement was inverted in the air for the purpose of removing every possible objection to the conclusions, but they came out exactly the same.

1224. Some results obtained with a brass hemisphere instead of the ball B were exceedingly interesting. It was 1·36 of an inch in diameter, (fig. 7.), and being placed on the top of the excited shell-lac cylinder, the carrier ball was applied, as in the former experiments (1218.), at the respective positions delineated in the figure. At *i* the force was 112°, at *k* 108°, at *l* 65°, at *m* 35°; the inductive force gradually diminishing, as might have been expected, to this point. But on raising the carrier to the position *n* the charge increased to 87°; and on raising it still higher to *a*, the charge still further increased to 105°: at a higher point still, *p*, the charge taken was smaller in amount, being 98°, and continued to diminish for more elevated positions. Here the induction fairly turned a corner. Nothing, in fact, can better show both the curved lines or courses of the inductive action, disturbed as they are from their rectilineal form by the shape, position, and condition of the metallic hemisphere; and also a *lateral tension*, so to speak, of these lines on one another:—all depending, as I conceive, on induction being an action of the contiguous particles of the dielectric thrown into a state of polarity and tension, and mutually related by their forces in all directions.

1225. As another proof that the whole of these actions were inductive I may state a result which was exactly what might be expected, namely, that if uninsulated conducting matter was brought round and near to the excited shell-lac stem, then the inductive force was directed towards it, and could not be found on the top of the hemisphere. Removing this matter the lines of force resumed their former direction. The experiment affords proofs of the lateral tension of these lines, and supplies a warning to remove such matter in repeating the above investigation.

1226. After these results on curved inductive action in air I extended the experiments to other gases, using first carbonic acid and then hydrogen: the phenomena were precisely those already described. In these experiments I found that if the gases were confined in vessels they required to be very large, for whether of glass or earthenware, the conducting power of such materials is so great that the induction of the excited shell-lac cylinder towards them is as much as if they were metal; and if the vessels be small, so great a portion of the inductive force is determined

towards them that the lateral tension or mutual repulsion of the lines of force before spoken of, (1224.) by which their inflexion is caused, is so much relieved in other directions, that no inductive charge will be given to the carrier ball in the positions *k*, *l*, *m*, *n*, *o*, *p*, (fig. 7.). A very good mode of making the experiment is to let large currents of the gases ascend or descend through the air, and carry on the experiments in these currents.

1227. These experiments were then varied by the substitution of a liquid dielectric, namely, *oil of turpentine*, in place of air and gases. A dish of thin glass well covered with a film of shell-lac, (1272.) and found by trial to insulate well, had some highly rectified oil of turpentine put into it to the depth of half an inch, and being then placed upon the top of the brass hemisphere, (fig. 7.) observations were made with the carrier ball as before (1224.). The results were the same, and the circumstance of some of the positions being within the fluid and some without, made no sensible difference.

1228. Lastly, I used a few solid dielectrics for the same purpose, and with the same results. These were shell-lac, sulphur, fused and cast borate of lead, flint glass well covered with a film of lac, and spermaceti. The following was the form of experiment with sulphur, and all were of the same kind. A square plate of the substance, two inches in extent and 0·6 of an inch in thickness, was cast with a small hole or depression in the middle of one surface to receive the carrier ball. This was placed upon the surface of the metal hemisphere (fig. 9.) arranged on the excited lac as in former cases, and observations were made at *n*, *o*, *p*, and *q*. Great care was required in these experiments to free the sulphur or other solid substance from any charge it might previously have received. This was done by breathing and wiping (1203.), and the substance being found free from all electrical excitement, was then used in the experiment; after which it was removed and again examined, to ascertain that it had received no charge, but had acted really as a dielectric. With all these precautions the results were the same: and it is thus very satisfactory to obtain the curved inductive action through *solid bodies*, as any possible effect from the translation of charged particles in fluids or gases, which some persons might imagine to be the case, is here entirely negatived.

1229. In these experiments with solid dielectrics, the degree of charge assumed by the carrier ball at the situations *n*, *o*, *p* (fig. 9.), was decidedly greater than that given to the ball at the same places when air only intervened between it and the metal hemisphere. This effect is consistent with what will hereafter be found to be the respective relations of these bodies, as to their power of facilitating induction through them (1269. 1273. 1277.).

1230. I might quote *many* other forms of experiment, some old and some new, in which induction in curved or contorted lines takes place, but think it unnecessary after the preceding results; I shall therefore mention but two. If a conductor A, (fig. 8.) be electrified, and an uninsulated metallic ball B, or even a plate, provided the edges be not too thin, be held before it, a small electrometer at *c* or at *d*, uninsu-

lated, will give signs of electricity, opposite in its nature to that of A, and therefore caused by induction, although the influencing and influenced bodies cannot be joined by a right line passing through the air. Or if, the electrometers being removed, a point be fixed at the back of the ball in its uninsulated state as at C, this point will become luminous and discharge the conductor A. The latter experiment is described by Nicholson*, who, however, reasons erroneously upon it. As to its introduction here, though it is a case of discharge, the discharge is preceded by induction, and that induction must be in curved lines.

1231. As argument against the received theory of induction and in favour of that which I have ventured to put forth, I cannot see how the preceding results can be avoided. The effects are clearly inductive effects produced by electricity, not in currents but in its statical state, and this induction is exerted in lines of force which, though in many experiments they may be straight, are here curved more or less according to circumstances. I use the term *line of inductive force* merely as a temporary conventional mode of expressing the direction of the power in cases of induction; and in the experiments with the hemisphere (1224.), it is curious to see how, when certain lines have terminated on the under surface and edge of the metal, those which were before lateral to them *expand and open out from each other*, some bending round and terminating their action on the upper surface of the hemisphere, and others meeting, as it were, above in their progress outwards, uniting their forces to give an increased charge in the carrier ball, at an *increased distance* from the source of power, and influencing each other so as to cause a second flexure in the contrary direction from the first one. All this appears to me to prove that the whole action is one of contiguous particles, related to each other, not merely in the lines which they may be conceived to form through the dielectric, between the inductric and the inducteous surfaces, but in other lateral directions also. It is this which gives the effect equivalent to lateral repulsion or expansion in the lines of force I have spoken of, and enables induction to turn a corner (1304.). The power, instead of being like that of gravity, which relates particles together through straight lines, whatever other particles may be between them, is more analogous to that of a series of magnetic needles, or to the condition of the particles considered as forming the whole of a straight or a curved magnet. So that in whatever way I view it, and with great suspicion of the influence of favourite notions over myself, I cannot perceive how the ordinary theory of induction can be a correct representation of that great natural principle of electrical action.

1232. I have had occasion in describing the precautions necessary in the use of the inductive apparatus, to refer to one founded on induction in curved lines (1203.); and after the experiments already described, it will easily be seen how great an influence the shell-lac stem may exert upon the charge of the carrier ball when applied to the apparatus (1218.), unless that precaution be attended to.

* Encyclopædia Britannica, vol. vi. p. 504.

1233. I think it expedient, next in the course of these experimental researches, to describe some effects due to *conduction*, obtained with such bodies as glass, lac, sulphur, &c., which had not been anticipated. Being understood, they will make us acquainted with certain precautions necessary in investigating the great question of specific inductive capacity.

1234. One of the inductive apparatus already described (1187, &c.) had a hemispherical cup of shell-lac introduced, which being in the interval between the inner ball and the lower hemisphere, nearly occupied the space there; consequently when the apparatus was charged, the lac was the dielectric or insulating medium through which the induction took place in that part. When this apparatus was first charged with electricity (1198.) up to a certain intensity, as 400°, measured by the Coulomb's electrometer (1180.), it sank much faster from that degree than if it had been previously charged to a higher point, and had gradually fallen to 400°; or than it would do if the charge were, by a second application, raised up again to 400°; all other things remaining the same. Again, if after having been charged for some time, as fifteen or twenty minutes, it was suddenly and perfectly discharged, even the stem having all electricity removed from it (1203.), then the apparatus being left to itself, would gradually recover a charge, which in nine or ten minutes would rise up to 50° or 60°, and in one instance to 80°.

1235. The electricity, which in these cases returned from an apparently latent to a sensible state, was always of the same kind as that which had been given by the charge. The return took place at both the inducing surfaces; for if after the perfect discharge of the apparatus the whole was insulated, as the inner ball resumed a positive state the outer sphere acquired a negative condition.

1236. This effect was at once distinguished from that produced by the excited stem acting in curved lines of induction (1203. 1232.), by the circumstance that all the returned electricity could be perfectly and instantly discharged. It appeared to depend upon the shell-lac within, and to be, in some way, due to electricity evolved from it in consequence of a previous condition into which it had been brought by the charge of the metallic coatings or balls.

1237. To examine this state more accurately, the apparatus, with the hemispherical cup of shell-lac in it, was charged for about forty-five minutes to above 600° with positive electricity at the balls *h* and B (fig. 1.) above and within. It was then discharged, opened, the shell-lac taken out, and its state examined; this was done by bringing the carrier ball near the shell-lac, uninsulating it, insulating it, and then observing what charge it had acquired. As it would be a charge by induction, the state of the ball would indicate the opposite state of electricity in that surface of the shell-lac which had produced it. At first the lac appeared quite free from any charge; but gradually its two surfaces assumed opposite states of electricity, the concave surface, which had been next the inner and positive ball, assuming a positive state, and the convex surface, which had been in contact with the negative

coating, acquiring a negative state; these states gradually increasing in intensity for some time.

1238. As the return action was evidently greatest instantly after the discharge, I again put the apparatus together, and charged it for fifteen minutes as before, the inner ball positively. I then discharged it, instantly removing the upper hemisphere with the interior ball, and, leaving the shell-lac cup in the lower uninsulated hemisphere, examined its inner surface by the carrier ball as before (1237.). In this way I found the surface of the shell-lac actually *negative*, or in the reverse state to the ball which had been in it; this state quickly disappeared, and was succeeded by a positive condition, gradually increasing in intensity for some time, in the same manner as before. This first negative condition of the surface opposite the positive charging ball is a natural consequence of the state of things, the charging ball being in contact with the shell-lac only in a few points. It does not interfere with the general result and peculiar state now under consideration, except that it assists in illustrating in a very marked manner the ultimate assumption by the surfaces of the shell-lac of an electrified condition, similar to that of the metallic surfaces opposed to or against them.

1239. *Glass* was then examined with respect to its power of assuming this peculiar state. I had a thick flint glass hemispherical cup formed, which would fit easily into the space *o* of the lower hemisphere (1188. 1189.); it had been heated and varnished with a solution of shell-lac in alcohol, for the purpose of destroying the conducting power of the vitreous surface. Being then well warmed and experimented with, I found it could also assume the *same state*, but not apparently to the same degree, the return action amounting in different cases to quantities from 6° to 18°.

1240. *Spermaceti* experimented with in the same manner gave striking results. When the original charge had been sustained for fifteen or twenty minutes at about 500°, the return charge was equal to 95° or 100°, and was about fourteen minutes arriving at the maximum effect. A charge continued for not more than two or three seconds was here succeeded by a return charge of 50° or 60°. The observations formerly made (1234.) held good with this substance. Spermaceti, though it will insulate a low charge for some time, is a better conductor than shell-lac, glass, and sulphur; and this conducting power is connected with its readiness in exhibiting the particular effect under consideration.

1241. *Sulphur*.—I was anxious to obtain the amount of effect with this substance, first, because it is an excellent insulator, and in that respect would illustrate the relation of the effect to the degree of conducting power possessed by the dielectric (1247.); and in the next place, that I might obtain that body giving the smallest degree of the effect now under consideration, for the investigation of the question of specific inductive capacity (1277.).

1242. With a good hemispherical cup of sulphur cast solid and sound, I obtained the return charge, but only to an amount of 17° or 18°. Thus glass and sulphur,

which are bodily very bad conductors of electricity, and indeed almost perfect insulators, gave very little of this return charge.

1243. I tried the same experiment having *air* only in the inductive apparatus. After a continued high charge for some time I could obtain a little effect of return action, but it was ultimately traced to the shell-lac of the stem.

1244. I sought to produce something like this state with one electric power and without induction; for upon the theory of an electric fluid or fluids, that did not seem impossible, and then I should have obtained an absolute charge (1169. 1177.), or something equivalent to it. In this I could not succeed. I excited the outside of a cylinder of shell-lac very highly for some time, and then quickly discharging it (1203.), waited and watched whether any return charge would appear, but such was not the case. This is another fact in favour of the inseparability of the two electric forces, and another argument for the view that induction and its concomitant phenomena depend upon a polarity of the particles of matter.

1245. Although inclined at first to refer these effects to a peculiar masked condition of a certain portion of the forces, I think I have since correctly traced them to known principles of electrical action. The effects appear to be due to an actual penetration of the charge to some distance within the electric, at each of its two surfaces, by what we call conduction; so that, to use the ordinary phrase, the electric forces sustaining the induction are not upon the metallic surfaces only, but upon and within the dielectric also, extending to a greater or smaller depth from the metal linings. Let *c* (fig. 10.) be the section of a plate of any dielectric, *a* and *b* being the metallic coatings; let *b* be uninsulated, and *a* be charged positively; after ten or fifteen minutes, if *a* and *b* be discharged, insulated, and immediately examined, no electricity will appear in them; but in a short time, upon a second examination, they will appear charged in the same way, though not to the same degree, as they were at first. Now suppose that a portion of the positive force has, under the coercing influence of all the forces concerned, penetrated the dielectric and taken up its place at the line *p*, a corresponding portion of the negative force having also assumed its position at the line *n*; that in fact the electric at these two parts has become charged positive and negative; then it is clear that the induction of these two forces will be much greater one towards the other, and less in an external direction, now that they are at the small distance *n p* from each other, than when they were at the larger interval *a b*. Then let *a* and *b* be discharged; the discharge destroys or neutralizes all external induction, and the coatings are therefore found by the carrier ball unelectrified; but it also removes almost the whole of the forces by which the electric charge was driven into the dielectric, and though probably a part goes forward in its passage and terminates in what we call discharge, the greater portion returns on its course to the surfaces of *c*, and consequently to the conductors *a* and *b*, and constitutes the recharge observed.

1246. The following is the experiment on which I rest for the truth of this view.

Two plates of spermaceti, *d* and *f* (fig. 11.), were put together to form the dielectric, *a* and *b* being the metallic coatings of this compound plate, as before. The system was charged, then discharged, insulated, examined, and found to give no indications of electricity to the carrier ball. The plates *d* and *f* were then separated from each other, and instantly *a* with *d* was found in a positive state, and *b* with *f* in a negative state, nearly all the electricity being in the linings *a* and *b*. Hence it is clear that, of the forces sought for, the positive was in one half of the compound plate and the negative in the other half; for when removed bodily with the plates from each other's inductive influence, they appeared in separate places, and resumed of necessity their power of acting by induction on the electricity of surrounding bodies. Had the effect depended upon a peculiar relation of the contiguous particles of matter only, then each half plate, *d* and *f*, should have shown positive force on one surface and negative on the other.

1247. Thus it would appear that the best solid insulators, such as shell-lac, glass, and sulphur, have conductive properties to such an extent, that electricity can penetrate them bodily, though always subject to the overruling condition of induction (1178.). As to the depth to which the forces penetrate in this form of charge of the particles, theoretically, it should be throughout the mass, for what the charge of the metal does for the portion of dielectric next to it, should be done by the charged dielectric for the portion next beyond it again; but probably in the best insulators the sensible charge is to a very small depth only in the dielectric, for otherwise more would disappear in the first instance whilst the original charge is sustained, less time would be required for the assumption of the particular state, and more electricity would re-appear as return charge.

1248. The condition of *time* required for this penetration of the charge is important, both as respects the general relation of the cases to conduction, and also the removal of an objection that might otherwise properly be raised to certain results respecting specific inductive capacities, hereafter to be given (1269. 1277.).

1249. It is the assumption for a time of this charged state of the glass between the coatings in the Leyden jar, which gives origin to a well-known phenomenon, usually referred to the diffusion of electricity over the uncoated portion of the glass, namely, the *residual charge*. The extent of charge which can spontaneously be recovered by a large battery, after perfect uninsulation of both surfaces, is very considerable, and by far the largest portion of this is due to the return of electricity in the manner described. A plate of shell-lac six inches square, and half an inch thick, or a similar plate of spermaceti an inch thick, being coated on the sides with tin foil as a Leyden arrangement, will show this effect exceedingly well.

1250. The peculiar condition of dielectrics which has now been described, is evidently capable of producing an effect interfering with the results and conclusions drawn from the use of the two inductive apparatus, when shell-lac, glass, &c. is used in one or both of them (1192. 1207.); for upon dividing the charge in such cases ac-

cording to the method described (1198. 1207.), it is evident that the one just receiving its half charge must fall faster in its tension than the other. For suppose app. i. first charged, and app. ii. used to divide with it; though both may actually lose alike, yet app. i., which has been diminished one half, will be sustained by a certain degree of return action or charge (1234.), whilst app. ii. will sink the more rapidly from the coming on of the particular state. I have endeavoured to avoid this interference by performing the whole process of comparison as quickly as possible, and taking the force of app. ii. immediately after the division, before any sensible diminution of the tension arising from the assumption of the peculiar state could be produced; and I have assumed that as about three minutes pass between the first charge of app. i. and the division, and three minutes between the division and discharge, when the force of the non-transferable electricity is measured, the contrary tendencies for those periods would keep that apparatus in a moderately steady and uniform condition for the latter portion of time.

1251. The particular action described occurs in the shell-lac of the stems, as well as in the *dielectric* used within the apparatus. It therefore constitutes a cause by which the outside of the stems may in some operations become charged with electricity, independent of the action of dust or carrying particles (1203.).

¶ v. *On specific induction, or specific inductive capacity.*

1252. I now proceed to examine the great question of specific inductive capacity, i. e. whether different dielectric bodies actually do possess any influence over the degree of induction which takes place through them. If any such difference should exist, it appeared to me not only of high importance in the further comprehension of the laws and results of induction, but an additional and very powerful argument for the theory I have ventured to put forth, that the whole depends upon a molecular action, in contradistinction to one at sensible distances.

The question may be stated thus: suppose A an electrified plate of metal suspended in the air, and B and C two exactly similar plates, placed parallel to and on each side of A at equal distances and uninsulated; A will then induce equally towards B and C. If in this position of the plates some other dielectric than air, as shell-lac, be introduced between A and C, will the induction between them remain the same? Will the relation of C and B to A be unaltered, notwithstanding the difference of the dielectrics interposed between them?

1253. As far as I recollect, it is assumed that no change will occur under such variation of circumstances, and that the relations of B and C to A depend entirely upon their distance. I only remember one experimental illustration of the question, and that is by COULOMB*, in which he shows that a wire surrounded by shell-lac took exactly the same quantity of electricity from a charged body as the same wire in air. The experiment offered to me no proof of the truth of the supposition, for it is not

* Mémoires de l'Académie, 1787, pp. 452, 453.

the mere films of dielectric substances surrounding the charged body which have to be examined and compared, but the *whole mass* between that body and the surrounding conductors at which the induction terminates. Charge depends upon induction (1171. 1178.); and if induction relate to the particles of the surrounding dielectric, then it relates to *all* the particles of that dielectric inclosed by the surrounding conductors, and not merely to the few situated next to the charged body. Whether the difference I sought for existed or not, I soon found reason to doubt the conclusion that might be drawn from Coulomb's result; and therefore had the apparatus made, which, with its use, has been already described (1187, &c.), and which appears to me well suited for the investigation of the question.

1254. Glass, and many bodies which might at first be considered as very fit to test the principle, proved exceedingly unfit for that purpose. Glass, principally in consequence of the alkali it contains, however well warmed and dried it may be, has a certain degree of conducting power upon its surface, dependent upon the moisture of the atmosphere, which renders it unfit for a test experiment. Resin, wax, naphtha, oil of turpentine, and many other substances were in turn rejected, because of a slight degree of conducting power possessed by them; and ultimately shell-lac and sulphur were chosen, after many experiments, as the dielectrics best fitted for the investigation. No difficulty can arise in perceiving how the possession of a feeble degree of conducting power tends to make a body produce effects, which would seem to indicate that it had a greater capability of allowing induction through it than another body perfect in its insulation. This source of error has been the one I have found most difficult to obviate in the proving experiments.

1255. *Induction through shell-lac.*—As a preparatory experiment, I first ascertained generally that when a part of the surface of a thick plate of shell-lac was excited or charged, there was no sensible difference in the character of the induction sustained by that charged part, whether exerted through the air in the one direction, or through the shell-lac of the plate in the other; provided the second surface of the plate had not, by contact with conductors, the action of dust, or any other means, become charged (1203.). Its solid condition enabled it to retain the excited particles in a permanent position, but that appeared to be all; for these particles acted just as freely through the shell-lac on one side as through the air on the other. The same general experiment was made by attaching a disc of tin foil to one side of the shell-lac plate, and electrifying it, and the results were the same. Scarcely any other solid substance than shell-lac and sulphur, and no liquid substance that I have tried, will bear this examination. Glass in its ordinary state utterly fails; yet it was essentially necessary to obtain this prior degree of perfection in the dielectric used, before any further progress could be made in the principal investigation.

1256. *Shell-lac and air* were compared in the first place. For this purpose a thick hemispherical cup of shell-lac was introduced into the lower hemisphere of one of the inductive apparatus (1187, &c.), so as nearly to fill the lower half of the space *o, o*

(fig. 1.) between it and the inner ball; and then charges were divided in the manner already described (1198. 1207.), each apparatus being used in turn to receive the first charge before its division by the other. As the apparatus were known to have equal inductive power when air was in both (1209. 1211.), any differences resulting from the introduction of the shell-lac would show a peculiar action in it, and if unequivocally referable to a specific inductive influence, would establish the point sought to be sustained. I have already referred to the precautions necessary in making the experiments (1199, &c.); and with respect to the error which might be introduced by the assumption of the peculiar state, it was guarded against, as far as possible, in the first place, by operating quickly (1248.), and afterwards by using that dielectric as glass or sulphur, which assumed the peculiar state most slowly, and in the least degree (1239. 1241.).

1257. The shell-lac hemisphere was put into app. i., and app. ii. left filled with air. The results of an experiment in which the charge through air was divided and reduced by the shell-lac app. were as follows:

App. i. Lac.	App. ii. Air.	
Balls 255°.		
0°		
. . . .	304°	
. . . .	297	
Charge divided.		
113		
. . . .	121	
0		after being discharged.
. . . .	7	after being discharged.

1258. Here 297°, minus 7°, or 290°, may be taken as the divisible charge of app. ii. (the 7° being fixed stem action (1203. 1232.)), of which 145° is the half. The lac app. i. gave 113° as the power or tension it had acquired after division; and the air app. ii. gave 121°, minus 7°, or 114°, as the force it possessed from what it retained of the divisible charge of 290°. These two numbers should evidently be alike, and they are very nearly so, far indeed within the errors of experiment and observation. But these numbers differ very much from 145°, or the force which the half charge would have had if app. i. had contained air instead of shell-lac; and it appears that whilst in the division the induction through the air has lost 176° of force, that through the lac has only gained 113°.

1259. If this difference be assumed as depending entirely on the greater facility possessed by shell-lac of allowing or causing inductive action through its substance than that possessed by air, then this capacity for electric induction would be inversely as the respective loss and gain indicated above; and assuming the capacity of the air apparatus as 1, that of the shell-lac apparatus would be $\frac{176}{113}$, or 1·55.

1260. This extraordinary difference was so unexpected in its amount, as to excite the greatest suspicion of the general accuracy of the experiment, though the perfect discharge of app. i. after the division showed that the 113° had been taken and given up readily. It was evident that, if it really existed, it ought to produce corresponding effects in the reverse order; and that when induction through shell-lac was converted into induction through air, the force or tension of the whole ought to be *increased.* The app. i. was therefore charged in the first place, and its force divided with app. ii. The following were the results:

App. i. Lac.	App. ii. Air.
. . . .	0°
215°	
204	
Charge divided.	
. . . .	118
118	
. . . .	0 after being discharged.
0	after being discharged.

1261. Here 204° must be the utmost of the divisible charge. The app. i. and ii. present 118° as their respective forces; both now much *above* the half of the first force, or 102°, whereas in the former case they were below it. The lac app. i. has lost only 86°, yet it has given to the air app. ii. 118°, so that the lac still appears much to surpass the air, the capacity of the lac app. i. to the air app. ii. being as 1·37 to 1.

1262. The difference of 1·55 and 1·37 as the expression of the capacity for the induction of shell-lac seems considerable, but is in reality very admissible under the circumstances, for both are in error in *contrary directions.* Thus in the last experiment the charge fell from 215° to 204° by the joint effects of dissipation and absorption (1192. 1250.), during the time which elapsed in the electrometer operations, between the applications of the carrier ball required to give those two results. Nearly an equal time must have elapsed between the application of the carrier which gave the 204° result, and the division of the charge between the two apparatus; and as the fall in force progressively decreases in amount (1192.), if in this case it be taken at 6° only, it will reduce the whole transferable charge at the time of division to 198° instead of 204°; this diminishes the loss of the shell-lac charge to 80° instead of 86°; and then the expression of specific capacity for it is increased, and, instead of 1·37, is 1·47 times that of air.

1263. Applying the same correction to the former experiment in which air was *first* charged, the result is of the *contrary* kind. No shell-lac hemisphere was then in the apparatus, and therefore the loss would principally be from dissipation, and not from absorption: hence it would be nearer to the degree of loss shown by the numbers 304° and 297°, and being assumed as 6° would reduce the divisible charge to 284°. In

that case the air would have lost 170°, and communicated only 113° to the shell-lac; and the relative specific capacity of the latter would appear to be 1·50, which is very little indeed removed from 1·47, the expression given by the second experiment when corrected in the same way.

1264. The shell-lac was then removed from app. i. and put into app. ii. and the experiments of division again made. I give the results, because I think the importance of the point justifies and even requires them.

App. i. Air.		App. ii. Lac.
	Balls 200°.	
	. . . .	0°
286°	. . . .	
283	. . . .	
	Charge divided.	
	. . . .	110
109	. . . .	
	. . . .	0·25 after discharge.
Trace	. . . .	after discharge.

Here app. i. retained 109°, having lost 174° in communicating 110° to app. ii.; and the capacity of the air app. is to the lac app., therefore, as 1 to 1·58. If the divided charge be corrected for an assumed loss of only 3°, being the amount of previous loss in the same time, it will make the capacity of the shell-lac app. 1·55 only.

1265. Then app. ii. was charged, and the charge divided thus:

App. i. Air.		App. ii. Lac.
0°	. . . .	
	. . . .	256°
	. . . .	251
	Charge divided.	
146	. . . .	
	. . . .	149
a little	. . . .	after discharge.
	. . . .	a little after discharge.

Here app. i. acquired a charge of 146°, while app. ii. lost only 102° in communicating that amount of force; the capacities being, therefore, to each other as 1 to 1·43. If the whole transferable charge be corrected for a loss of 4° previous to division, it gives the expression of 1·49 for the capacity of the shell-lac apparatus.

1266. These four expressions of 1·47, 1·50, 1·55, and 1·49 for the power of the shell-lac apparatus, through the different variations of the experiment, are very near to each other; the average is close upon 1·5, which may hereafter be used as the expression of the result. It is a very important result; and, showing for this parti-

cular piece of shell-lac a decided superiority over air in allowing or causing the act of induction, it proved the growing necessity of a more close and rigid examination of the whole question.

1267. The shell-lac was of the best quality, and had been carefully selected and cleaned; but as the action of any conducting particles in it would tend, virtually, to diminish the quantity or thickness of the dielectric used, and produce effects as if the two inducing surfaces of the conductors in that apparatus were nearer together than in the one with air only, I prepared another shell-lac hemisphere, of which the material had been dissolved in strong spirit of wine, the solution filtered, and then carefully evaporated. This is not an easy operation, for it is difficult to drive off the last portions of alcohol without injuring the lac by the heat applied; and unless they be dissipated, the substance left conducts too well to be used in these experiments. I prepared two hemispheres this way, one of them unexceptionable; and with it I repeated the former experiments with all precautions. The results were exactly of the same kind; the following expressions for the capacity of the shell-lac apparatus, whether it were app. i. or ii., being given directly by the experiments 1·46, 1·50, 1·52, 1·51; the average of these and several others being very nearly 1·5.

1268. As a final check upon the general conclusion, I then actually brought the surfaces of the air apparatus, corresponding to the place of the shell-lac in its apparatus, nearer together, by putting a metallic lining into the lower hemisphere of the one not containing the lac (1213.). The distance of the metal surface from the carrier ball was in this way diminished from 0·62 of an inch to 0·435 of an inch, whilst the interval occupied by the lac in the other apparatus remained 0·62 of an inch as before. Notwithstanding this change, the lac apparatus showed its former superiority; and whether it or the air apparatus was charged first, the capacity of the lac apparatus to the air apparatus was by the experimental results as 1·45 to 1.

1269. From all the experiments I have made, and their constant results, I cannot resist the conclusion that shell-lac does exhibit a case of *specific inductive capacity.* I have tried to check the trials in every way, and if not remove, at least estimate, every source of error. That the final result is not due to common conduction is shown by the capability of the apparatus to retain the communicated charge; that it is not due to the conductive power of inclosed small particles, by which they could acquire a polarized condition as conductors, is shown by the effects of the shell-lac purified by alcohol; and, that it is not due to any influence of the charged state, formerly described (1250.), first absorbing and then evolving electricity, is indicated by the *instantaneous* assumption and discharge of those portions of the power which are concerned in the phenomena, that effect occurring in these cases, as in all others of ordinary induction by charged conductors. The latter argument is the more striking in the case where the air apparatus is employed to divide the charge with the lac apparatus, for it obtains its portion of electricity in an *instant,* and yet is charged far above the *mean.*

1270. Admitting for the present the general fact sought to be proved; then 1·5, though it expresses the capacity of the apparatus containing the hemisphere of shell-lac, by no means expresses the relation of lac to air. The lac only occupies one half of the space *o, o,* of the apparatus containing it, through which the induction is sustained; the rest is filled with air, as in the other apparatus; and if the effect of the two upper halves of the globes be abstracted, then the comparison of the shell-lac powers in the lower half of the one, with the power of the air in the lower half of the other, will be as 2 : 1; and even this must be less than the truth, for the induction of the upper part of the apparatus, i. e. of the wire and ball B (fig. 1.) to external objects, must be the same in both, and considerably diminish the difference dependent upon, and really producible by, the influence of the shell-lac within.

1271. *Glass.*—I next worked with glass as the dielectric. It involved the possibility of conduction on its surface, but it excluded the idea of conducting particles within its substance (1267.) other than those of its own mass. Besides this it does not assume the charged state (1239.) so readily, or to such an extent as shell-lac.

1272. A thin hemispherical cup of glass being made hot was covered with a coat of shell-lac dissolved in alcohol, and after being dried for many hours in a hot place, was put into the apparatus and experimented with. It exhibited effects so slight, that, though they were in the direction indicating a superiority of glass over air, they were allowed to pass as possible errors of experiment; and the glass was considered as producing no sensible effect.

1273. I then procured a thick flint glass hemispherical cup resembling that of shell-lac (1239.), but not filling up the space *o, o,* so well. Its average thickness was 0·4 of an inch, there being an additional thickness of air, averaging 0·22 of an inch, to make up the whole space of 0·62 of an inch between the inducting metallic surfaces. It was covered with a film of shell-lac as the former was, (1272.) and being made very warm, was introduced into the apparatus, also warmed, and experiments made with it as in the former instances (1257. &c.). The general results were the same as with shell-lac, i. e. glass surpassed air in its power of favouring induction through it. The two best results as respected the state of the apparatus for retention of charge, &c., gave, when the air apparatus was charged first 1·336, and when the glass apparatus was charged first 1·45, as the specific inductive capacity for glass, both being without correction. The average of nine results, four with the glass apparatus first charged, and five with the air apparatus first charged, gave 1·38 as the power of the glass apparatus; 1·22 and 1·46 being the minimum and maximum numbers with all the errors of experiment upon them. In all the experiments the glass apparatus took up its inductive charge instantly, and lost it as readily; and during the short time of each experiment, acquired the peculiar state in a small degree only, so that the influence of this state, and also of conduction upon the results, must have been small.

1274. Allowing specific inductive capacity to be proved and active in this case, and

1·38 as the expression for the glass apparatus, then the specific inductive capacity of flint glass will be above 1·76, not forgetting that this expression is for a piece of glass of such thickness as to occupy not quite two-thirds of the space through which the induction is sustained (1273. 1253.).

1275. *Sulphur.*—The same hemisphere of this substance was used in app. ii. as was formerly referred to (1242.). The experiments were well made, i. e. the sulphur itself was free from charge both before and after each experiment, and no action from the stem appeared (1203. 1232.), so that no correction was required on that score. The following are the results when the air apparatus was first charged and divided:

App. i. Air.		App. ii. Sulphur.
	Balls 280°.	
0°	. . . .	
	. . . .	0°
438	. . . .	
434	. . . .	
	Charge divided.	
	. . . .	162
164	. . . .	
	. . . .	160
162	. . . .	
	. . . .	0 after discharge.
0	. . . .	after discharge.

Here app. i. retained 164°, having lost 270° in communicating 162° to app. ii., and the capacity of the air apparatus is to that of the sulphur apparatus as 1 to 1·66.

1276. Then the sulphur apparatus was charged first, thus:

	. . . .	0°
0°	. . . .	
	. . . .	395
	. . . .	388
	Charge divided.	
237	. . . .	
	. . . .	238
0	. . . .	after discharge.
	. . . .	0 after discharge.

Here app. ii. retained 238°, and gave up 150° in communicating a charge of 237° to app. i., and the capacity of the air apparatus is to that of the sulphur apparatus as 1 to 1·58. These results are very near to each other, and we may take the mean 1·62 as representing the specific inductive capacity of the sulphur apparatus; in which

case the specific inductive capacity of sulphur itself as compared to air = 1 (1270.) will be about or above 2·24.

1277. This result with sulphur I consider as one of the most unexceptionable. The substance when fused was perfectly clear, pellucid, and free from particles of dirt (1267.), so that no interference of small conducting particles confused the result. The body when solid is an excellent insulator, and by experiment was found to take up, with great slowness, that state (1241. 1242.) which alone seemed likely to disturb the conclusion. The experiments themselves, also, were free from any need of correction. Yet notwithstanding these circumstances, so favourable to the exclusion of error, the result is a higher specific inductive capacity for sulphur than for any other body as yet tried; and though this may in part be due to the sulphur being in a better shape, i. e. filling up more completely the space *o*, *o*, (fig. 1.) than the cups of shell-lac and glass, still I feel satisfied that the experiments altogether fully prove the existence of a difference between dielectrics as to their power of favouring an inductive action through them; which difference may, for the present, be expressed by the term *specific inductive capacity*.

1278. Having thus established the point in the most favourable cases that I could anticipate, I proceeded to examine other bodies amongst solids, liquids, and gases. These results I shall give with all convenient brevity.

1279. *Spermaceti.*—A good hemisphere of spermaceti being tried as to conducting power whilst its two surfaces were still in contact with the tin foil moulds used in forming it, was found to conduct sensibly even whilst warm. On removing it from the moulds and using it in one of the apparatus, it gave results indicating a specific inductive capacity between 1·3 and 1·6 for the apparatus containing it. But as the only mode of operation was to charge the air apparatus, and then after a quick contact with the spermaceti apparatus, ascertain what was left in the former (1281.), no great confidence can be placed in the results. They are not in opposition to the general conclusion, but cannot be brought forward as argument in favour of it.

1280. I endeavoured to find some liquids which would insulate well, and could be obtained in sufficient quantity for these experiments. Oil of turpentine, native naphtha rectified, and the condensed oil gas fluid, appeared by common experiments to promise best as to insulation. Being left in contact with fused carbonate of potassa, chloride of lime, and quick lime for some days and then filtered, they were found much injured in insulating power; but after distillation acquired their best state, though even then they proved to be conductors when large metallic contact was made with them.

1281. *Oil of turpentine rectified.*—I filled the lower half of app. i. with the fluid; and as it would not hold a charge sufficiently to enable me first to measure and then divide it, I charged app. ii. containing air, and dividing its charge with app. i. by a quick contact, measured that remaining in app. ii.: for, theoretically, if a quick contact would divide up to equal tension between the two apparatus, yet without sen-

sible loss from the conducting power of app. i.; and app. ii. were left charged to a degree of tension above half the original charge, it would indicate that oil of turpentine had less specific inductive capacity than air; or, if left charged below that mean state of tension, it would imply that the fluid had the greater inductive capacity. In an experiment of this kind, app. ii. gave as its charge 390° before division with app. i., and 175° afterwards, which is less than the half of 390°. Again, being at 175° before division, it was 79° after, which is also less than half the divided charge. Being at 79°, it was a third time divided, and then fell to 36°, less than the half of 79°. Such are the best results I could obtain; they are not inconsistent with the belief that oil of turpentine has a greater specific capacity than air, but they do not prove the fact, since the disappearance of more than half the charge may be due to the conducting power merely of the fluid.

1282. *Naphtha.*—This liquid gave results similar in their nature and direction to those with oil of turpentine.

1283. A most interesting class of substances, in relation to specific inductive capacity, now came under review, namely, the gases or aeriform bodies. These are so peculiarly constituted, and are bound together by so many striking physical and chemical relations, that I expected some remarkable results from them: air in various states was selected for the first experiments.

1284. *Air, rare and dense.*—Some experiments of division (1208.) seemed to show that dense and rare air were alike in the property under examination. A simple and better process was to attach one of the apparatus to an air pump, to charge it, and then examine the tension of the charge when the air within was more or less rarefied. Under these circumstances it was found, that commencing with a certain charge, that charge did not change in its tension or force as the air was rarefied, until the rarefaction was such that *discharge* across the space *o, o* (fig. 1.) occurred. This discharge was proportionate to the rarefaction; but having taken place, and lowered the tension to a certain degree, that degree was not at all affected by restoring the pressure and density of the air to their first quantities.

	inches of mercury.		
Thus at a pressure of . . .	30	the charge was . . .	88°
Again	30	the charge was . . .	88
Again	30	the charge was . . .	87
Reduced to	14	the charge was . . .	87
Raised again to	30	the charge was . . .	86
Being now reduced to . . .	3·4	the charge fell to . . .	81
Raised again to	30	the charge was still . .	81

1285. The charges were low in these experiments, first that they might not pass off at low pressure, and next that little loss by dissipation might occur. I now reduced them still lower, that I might rarefy further, and for this purpose in the following experiment used a measuring interval in the electrometer of only 15° (1185.). The pressure of air within the apparatus being reduced to 1·9 inches of mercury,

the charge was found to be 29°; then letting in air till the pressure was 30 inches, the charge was still 29°.

1286. These experiments were repeated with pure oxygen with the same consequences.

1287. This result of *no variation* in the electric tension being produced by variation in the density or pressure of the air, agrees perfectly with those obtained by Mr. Harris*, and described in his beautiful and important investigations contained in the Philosophical Transactions; namely that induction is the same in rare and dense air, and that the divergence of an electrometer under such variations of the air continues the same, provided no electricity pass away from it. The effect is one entirely independent of that power which dense air has of causing a higher charge to be retained upon the surface of conductors in it than can be retained by the same conductors in rare air; a point I propose considering hereafter.

1288. I then compared *hot and cold air* together, by raising the temperature of one of the inductive apparatus as high as it could be without injury, and then dividing charges between it and the other apparatus containing cold air. The temperatures were about 50° and 200°. Still the power or capacity appeared to be unchanged; and when I endeavoured to vary the experiment, by charging a cold apparatus and then warming it by a spirit lamp, I could obtain no proof that the inductive capacity underwent any alteration.

1289. I compared *damp and dry air* together, but could find no difference in the results.

1290. *Gases.*—A very long series of experiments was then undertaken for the purpose of comparing *different gases* one with another. They were all found to insulate well, except such as acted on the shell-lac of the supporting stem; these were chlorine, ammonia, and muriatic acid. They were all dried by appropriate means before being introduced into the apparatus. It would have been sufficient to have compared each with air; but, in consequence of the striking result which came out, namely, that *all had the same power of,* or *capacity for,* sustaining induction through them, (which perhaps might have been expected after it was found that no variation of density or pressure produced any effect,) I was induced to compare them, experimentally, two and two in various ways, that no difference might escape me, and that the sameness of result might stand in full opposition to the contrast of property, composition, and condition which the gases themselves presented.

1291. The experiments were made upon the following pairs of gases.

1. Nitrogen and . . Oxygen.
2. Oxygen . . . . Air.
3. Hydrogen . . . Air.
4. Muriatic acid gas .. Air.
5. Oxygen . . . . Hydrogen.
6. Oxygen . . . . Carbonic acid.

* Philosophical Transactions, 1834, pp. 223, 224, 237, 244.

7. Oxygen	Olefiant gas.
8. Oxygen	Nitrous gas.
9. Oxygen	Sulphurous acid.
10. Oxygen	Ammonia.
11. Hydrogen. . . .	Carbonic acid.
12. Hydrogen. . . .	Olefiant gas.
13. Hydrogen. . . .	Sulphurous acid.
14. Hydrogen. . . .	Fluo-silicic acid.
15. Hydrogen. . . .	Ammonia.
16. Hydrogen. . . .	Arseniuretted hydrogen.
17. Hydrogen. . . .	Sulphuretted hydrogen.
18. Nitrogen	Olefiant gas.
19. Nitrogen	Nitrous gas.
20. Nitrogen	Nitrous oxide.
21. Nitrogen	Ammonia.
22. Carbonic oxide . .	Carbonic acid.
23. Carbonic oxide . .	Olefiant gas.
24. Nitrous oxide . .	Nitrous gas.
25. Ammonia	Sulphurous acid.

1292. Notwithstanding the striking contrasts of all kinds which these gases present of property, of density, whether simple or compound, anions or cathions (665.), of high or low pressure (1284. 1286.), hot or cold (1288.), not the least difference in their capacity to favour or admit electrical induction through them could be perceived. Considering the point established, that in all these gases induction takes place by an action of contiguous particles, this is the more important, and adds one to the many striking relations which hold between bodies having the gaseous condition and form. Another equally important electrical relation, which will be examined in the next paper, is that which the different gases have to each other at the *same pressure* of causing the retention of the *same or different degrees of charge* upon conductors in them. These two results appear to bear importantly upon the subject of electro-chemical excitation and decomposition; for as *all* these phenomena, different as they seem to be, must depend upon the electrical forces of the particles of matter, the very distance at which they seem to stand from each other will do much, if properly considered, to illustrate the principle by which they are held in one common bond, and subject, as they must be, to one common law.

1293. It is just possible that the gases may differ from each other in their specific inductive capacity, and yet by quantities so small as not to be distinguished in the apparatus I have used. It must be remembered, however, that in the gaseous experiments the gases occupy all the space *o*, *o*, (fig. 1.) between the inner and the outer ball, except the small portion filled by the stem; and the results, therefore, are twice as delicate as those with solid dielectrics.

1294. The insulation was good in all the experiments recorded, except Nos. 10, 15, 21, and 25, being those in which ammonia was compared with other gases. When shell-lac is put into ammoniacal gas its surface gradually acquires conducting power, and in this way the lac part of the stem within was so altered, that the ammonia apparatus could not retain a charge with sufficient steadiness to allow of division. In these experiments, therefore, the other apparatus was charged; its charge measured and divided with the ammonia apparatus by a quick contact, and what remained untaken away by the division again measured (1281.). It was so nearly one half of the original charge, as to authorize, with this reservation, the insertion of ammoniacal gas amongst the other gases, as having equal power with them.

1295. Thus *induction* appears to be essentially an action of contiguous particles, through the intermediation of which the electric force, originating or appearing at a certain place, is propagated to or sustained at a distance, appearing there as a force of the same kind exactly equal in amount, but opposite in its direction and tendencies (1164.). Induction requires no sensible thickness in the conductors which may be used to limit its extent; an uninsulated leaf of gold may be made very highly positive on one surface, and as highly negative on the other, without the least interference of the two states whilst the inductions continue. Nor is it affected by the nature of the limiting conductors, provided time be allowed, in the case of those which conduct slowly, for them to assume their final state (1170.).

1296. But with regard to the *dielectrics* or insulating media, matters are very different (1167.). Their thickness has an immediate and important influence on the degree of induction. As to their quality, though all gases and vapours are alike, whatever their state, amongst solid bodies, and between them and gases, there are differences which prove the existence of *specific inductive capacities*, these differences being in some cases very great.

1297. The direct inductive force, which may be conceived to be exerted in lines between the two limiting and charged conducting surfaces, is accompanied by a lateral or transverse force equivalent to a dilatation or repulsion of these representative lines (1224.); or the attractive force which exists amongst the particles of the dielectric in the direction of the induction is accompanied by a repulsive or a diverging force in the transverse direction (1304.).

1298. Induction appears to consist in a certain polarized state of the particles, into which they are thrown by the electrified body sustaining the action, the particles assuming positive and negative points or parts, which are symmetrically arranged with respect to each other and the inducting surfaces or particles*. The state must be a

* The theory of induction which I am stating does not pretend to decide whether electricity be a fluid or fluids, or a mere power or condition of recognised matter. That is a question which I may be induced to consider in the next or following series of these researches.

forced one, for it is originated and sustained only by force, and sinks to the normal or quiescent state when that force is removed. It can be *continued* only in insulators by the same portion of electricity, because they only can retain this state of the particles (1304.).

1299. The principle of induction is of the utmost generality in electric action. It constitutes charge in every ordinary case, and probably in every case; it appears to be the cause of all excitement, and to precede every current. The degree to which the particles are affected in this their forced state, before discharge of one kind or another supervenes, appears to constitute what we call *intensity*.

1300. When a Leyden jar is *charged*, the particles of the glass are forced into this polarized and constrained condition by the electricity of the charging apparatus. *Discharge* is the return of these particles to their natural state from their state of tension, whenever the two electric forces are allowed to be disposed of in some other direction.

1301. All charge of conductors is on their surface, because being essentially inductive, it is there only that the medium capable of sustaining the necessary inductive state begins. If the conductors are hollow and contain air or any other dielectric, still no *charge* can appear upon that internal surface, because the dielectric there cannot assume the polarized state throughout, in consequence of the opposing actions in different directions.

1302. The known influence of *form* is perfectly consistent with the corpuscular view of induction set forth. An electrified cylinder is more affected by the influence of the surrounding conductors (which complete the condition of charge) at the ends than at the middle, because the ends are exposed to a greater sum of inductive forces than the middle; and a point is brought to a higher condition than a ball, because, by relation to the conductors around, more inductive force terminates on its surface than on an equal surface of the ball with which it is compared. Here too, especially, can be perceived the influence of the lateral or transverse force (1297.), which, being a power of the nature of or equivalent to repulsion, causes such a disposition of the lines of inductive force in their course across the dielectric, that they must accumulate upon the point, the end of the cylinder, or any projecting part.

1303. The influence of *distance* is also in harmony with the same view. There is perhaps no distance so great that induction cannot take place through it*; but with the same constraining force (1298.) it takes place the more easily, according as the extent of dielectric through which it is exerted is lessened. And as it is assumed by the theory that the particles of the dielectric, though tending to remain in a normal state, are thrown into a forced condition during the induction; so it would seem to

* I have traced it experimentally from a ball placed in the middle of the large cube formerly described (1173.) to the sides of the cube six feet distant, and also from the same ball placed in the middle of our large lecture-room to the walls of the room at twenty-six feet distance, the charge upon the ball in these cases being solely due to induction through these distances.

follow that the fewer there are of these intervening particles opposing their tendency to the assumption of the new state, the greater degree of change will they suffer, i. e. the higher will be the condition they assume, and the larger the amount of inductive action exerted through them.

1304. I have used the phrases *lines of inductive force* and *curved lines* of force (1231. 1297. 1298. 1302.) in a general sense only, just as we speak of the lines of magnetic force. The lines are imaginary, and the force in any part of them is of course the resultant of compound forces, every molecule being related to every other molecule in *all* directions by the tension and reaction of those which are contiguous. The transverse force is merely this relation considered in a direction oblique to the lines of inductive force, and at present I mean no more than that by the phrase. With respect to the term *polarity* also, I mean at present only a disposition of force by which the same molecule acquires opposite powers on different parts. The particular way in which this disposition is made will come into consideration hereafter, and probably varies in different bodies, and so produces variety of electrical relation. All I am anxious about at present is, that a more particular meaning should not be attached to the expressions used than I contemplate. Further inquiry, I trust, will enable us by degrees to restrict the sense more and more, and so render the explanation of electrical phenomena day by day more and more definite.

1305. As a test of the probable accuracy of my views, I have throughout this experimental examination compared them with the conclusions drawn by M. Poisson from his beautiful mathematical inquiries*. I am quite unfit to form a judgment of these admirable papers; but as far as I can perceive, the theory I have set forth and the results I have obtained are not in opposition to such of those conclusions as represent the final disposition and state of the forces in the limited number of cases he has considered. His theory assumes a very different mode of action in induction to that which I have ventured to support, and would probably find its mathematical test in the endeavour to apply it to cases of induction in curved lines. To my feeling it is insufficient in its mode of accounting for the retention of electricity upon the surface of conductors by the pressure of the air, an effect which I hope to show is simple and consistent according to the present view; and it does not touch voltaic electricity, or in any way associate it and what is called ordinary electricity under one common principle.

I have also looked with some anxiety to the results which that indefatigable philosopher Harris has obtained in his investigation of the laws of induction†, knowing that they were experimental, and having a full conviction of their exactness; but I am happy in perceiving no collision at present between them and the views I have set forth.

1306. Finally, I beg to say that I put forth my particular view with doubt and fear,

* Mémoires de l'Institut, 1811, tom. xii. the first page 1, and the second paging 163.

† Philosophical Transactions, 1834, p. 213.

lest it should not bear the test of general examination, for unless true it will only embarrass the progress of electrical science. It has long been on my mind, but I hesitated to publish it until the increasing persuasion of its accordance with all known facts, and the manner in which it linked together effects apparently very different in kind, urged me to write the present paper. I as yet see no inconsistency between it and nature, but, on the contrary, think I perceive much new light thrown by it on her operations; and my next papers will be devoted to a review of the phenomena of conduction, electrolyzation, current, magnetism, retention, discharge, and some other points, with an application of the theory to these effects, and an examination of it by them.

Royal Institution,
November 16, 1837.

II. *Fourth Letter on Voltaic Combinations, with reference to the Mutual Relations of the Generating and Conducting Surfaces. Addressed to* Michael Faraday, *Esq. D.C.L. F.R.S., Fullerian Prof. Chem. Royal Institution, &c. &c. &c. By* J. Frederic Daniell, *F.R.S., Prof. Chem. in King's College, London.*

Received January 18,—Read January 25, 1838.

My dear Faraday,

In my second* letter to you upon Voltaic Combinations, I suggested that, in a theoretical point of view, the most simple and perfect combination would probably consist of a solid sphere (or rather active point) of a generating metal, surrounded by a hollow sphere of an inactive conducting metal, with an intervening liquid electrolyte; the circuit being completed by a conducting wire properly disposed for connecting the two metals. Further reflection led me to believe that a series of experiments commencing as nearly as possible with these most simple conditions of the problem, might throw some light upon the relative dimensions, positions and actions of the generating and conducting plates of voltaic combinations in general; which appeared to me not to have received all the elucidation of which they might be susceptible. The subject, it is true, has not escaped the attention of experimenters; but most of the results with which I am acquainted are so involved in the errors arising from the variable condition of the source of the current itself, which I have already pointed out, as to leave it in a very unsatisfactory state. I moreover proposed to myself, in the investigation which I consequently undertook, to trace the self-distribution of the force from its origin, by the indications of reduced copper, in the manner described in my first letter†. I have thus been led to some results which I trust you will consider of sufficient importance to justify my troubling you with another communication upon the subject.

My first apparatus, which it is necessary to describe, consists of two hollow hemispheres of brass, fitting together water-tight by means of exterior flanges half an inch wide, and a collar of leather, and thus forming a sphere, the interior diameter of which is $9\frac{1}{4}$ inches; consequently exposing a surface of about 268·8 square inches. The lower hemisphere is fitted into a frame carrying buttons, by which the upper can be securely wedged down upon it; and underneath there is a small cock, by which any liquid in the interior may be drawn off. The upper part of the upper hemisphere terminates in a tube of about one inch in length, forming an opening through which a membranous bag may be introduced, and from which it may be suspended. The

* Philosophical Transactions, 1836, p. 128.

† Ibid. p. 113.

electrolyte which I employed in all the following experiments was the same as that which I employ in the constant battery, viz. eight parts of water, by measure, to one part of oil of vitriol in contact with the generating surface, and the same diluted acid, further saturated with sulphate of copper, in contact with the conducting surface.

The measures of the force to which I had recourse were, in the first instance, a coarse galvanometer consisting of a single needle seven inches in length, with a coil of twenty turns of wire $\frac{1}{20}$th of an inch in diameter. The large scale of the experiments precluded me from employing delicate instruments with astatic needles. Being dissatisfied with this measure, I afterwards employed a Bréguet's thermometer fitted up according to the plan of Professor De la Rive*, which measures the force of a current which passes through it by the differences of its heating power upon the compound spiral of platinum and silver of which it consists, the degrees into which it is divided being directly proportional to such differences. I had every reason to be satisfied with this instrument; and have no doubt of its accuracy, in the case, at least, of currents of such low tension as those for which alone I have hitherto employed it.

In my first experiment the sphere was charged with the solution of copper, and the membrane with the plain acid; a small sphere of amalgamated zinc, one inch diameter (and exposing a surface therefore of 3·14 square inches), was suspended by means of a well-varnished copper wire in the centre of the latter: the other extremity of the wire was connected with one of the cups of the galvanometer, and the circuit was completed by a wire leading from the other cup to a small mercury cup upon the upper brass hemisphere, placed at a distance of two inches from the tube. The deviation of the needle was 60°; and it remained steady for a long period, during which the experiment was repeated and varied. When, instead of the galvanometer, the circuit was closed with a piece of platinum wire one inch in length and $\frac{1}{150}$ inch diameter, it continued red hot for a period of five hours. The circuit remained closed for seventeen hours, and the apparatus was then opened and examined. The zinc ball had dropped off the wire, and was reduced to about one half of its original size. The upper hemisphere was found coated with reduced copper, beautifully marked half-way up from the equator with concentric circles of alternate dark and light stripes of pink and red; these were followed by a broad even band of pink, which reached to a circle within $1\frac{1}{2}$ inch of the aperture, which was composed of the unchanged surface of the brass, and which evidently had not been in contact with the liquid. The lower hemisphere, which had been insulated from the upper by the collar of leather, had no copper precipitated upon it.

The sphere was again put together, and charged as before, with a new zinc ball. The circuit was closed, as in the first experiment, with the galvanometer in contact with the upper hemisphere, and the deviation of the needle was 60°.

The connexion was then broken with the upper hemisphere, and made with the bottom of the lower hemisphere; the deviation was again 60°.

* Mémoires de la Soc. de Phys. de Génève, 1836, p. 140.

When connexion was made at the same time with both hemispheres, the deviation was the same. If, while in this state, either wire were lifted singly from its connexion, the needle remained perfectly steady.

When the wire from the zinc ball was lifted from the cup, there was a spark upon breaking the circuit; and there was also a spark when the wires from the two hemispheres were disconnected from the circuit together, but not when lifted singly.

Upon leading the conducting wire of the lower hemisphere into direct communication with that of the zinc ball, while the wire of the upper hemisphere was connected as before with the galvanometer, or *vice versâ*, the needle only receded to 40°. From this it appeared that, although the whole amount of force originating at the zinc was capable of passing off by means of either hemisphere singly, when both passages were open it distributed itself between the two, notwithstanding the additional resistance in one by the interposition of the galvanometer.

Extra contacts made with different parts of the two hemispheres made no difference in any of the preceding experiments.

The apparatus was now left for eighteen hours with both hemispheres in connexion with one extremity of the galvanometer, and the zinc ball with the other. Upon examination the needle was found still deflected 25°; and upon agitation of the zinc ball it rose to 55°, but almost immediately declined to 25°. The acid had become nearly saturated with oxide of zinc. The sphere was opened, and both hemispheres exhibited a perfectly even coating of beautiful pink copper, through which the rings of the upper hemisphere were however still discernible.

It will perhaps be advantageous, before I proceed further, to collect into one point of view the principal conclusions which, I conceive, may be drawn from the preceding experiments with the spherical combination.

1st. The force emanating from the active zinc centre diffused itself over every part of the upper hemisphere, from which there was a good conducting passage for its circulation, to an amount which is measured by 60° of the galvanometer; and which was capable of maintaining an inch of platinum wire $\frac{1}{150}$ inch diameter at red heat.

2nd. The same amount of force was maintained by either hemisphere indifferently; but when both conducting hemispheres were in metallic communication there was no increase of force. The transfer of half the power from one hemisphere to the other occupied no appreciable interval of time.

3rd. Although the force was not increased it spread itself equally over the whole sphere, as manifested by the diffusion of the precipitated copper.

4th. When one hemisphere was connected with the zinc centre by a short wire capable of affording circulation to the whole force, and the other hemisphere was connected by a long wire through the galvanometer with the same centre, the diffusion of the force over the whole sphere was maintained, although the half of it was obliged to overcome the much greater resistance of the longer circuit.

5th. There was no greater accumulation of precipitated copper about the points

with which the conducting wires were brought into contact, and towards which the force diffused over the sphere must have converged, than at any other point; proving that the force must have diverged from the centre equally through the electrolyte, and could only have drawn towards the conducting wires in the conducting sphere itself.

I now destroyed the insulation of the two hemispheres by fixing to the lower one a thin ring of brass, which came in contact with the upper when wedged down in its position by the means already described. The sphere, thus in good metallic communication in every part, was charged as before; the precipitated copper having been previously cleaned off by a little nitric acid and rotten-stone. The deviation of the galvanometer was 55°, and it was perfectly steady, whatever number of connexions were made with the sphere, or at whatever point the circuit was completed. It made no difference in the amount of the force which circulated, whether the contact with the brass was made as near as possible to the zinc conducting wire, or at the point the farthest removed from it.

I next proceeded to ascertain what would be the effect of increasing the surface of the generating metal. For this purpose two amalgamated zinc balls of the same diameter as before were placed in contact upon a varnished wire, and substituted for the single ball in the centre of the sphere: the deviation of the galvanometer only increased 5°, rising from 55° to 60°. Upon replacing the single ball it fell again to 55°, and so alternately rose and fell to the same amount upon frequent repetitions of the change. An amalgamated zinc rod six inches in length, and $\frac{1}{2}$ inch in diameter, was then substituted for the balls, but the deviation did not rise higher than 60°. Thus the generating surface was increased from about three square inches to six and 9·4 with very little increased effect.

These experiments were all made with the generating metal placed as nearly as possible in the centre of the conducting sphere: this position was now changed, and sometimes the zinc ball was placed at the bottom of the membrane almost in contact with the sphere; sometimes it was drawn up nearly to the top; again it was placed in the centre; but none of these changes produced any appreciable alteration in the deviation of the needle, the galvanometer constantly indicating 55°.

From this I was led to consider (although I am quite aware that the measures may not have been sufficiently accurate to determine the point with precision,) whether the force emanating from the zinc ball might not diffuse itself over the surrounding conducting sphere in obedience to the well-known law of radiant forces of the inverse square of the distance; since, although the cases may be dissimilar, according to this law an attractive point placed within a hollow sphere of attractive matter remains in equilibrio, whatever its position may be. Or the analogy, perhaps, is stronger of light diffusing itself from a luminous point within the sphere in the same relative positions as those of the generating ball.

This conclusion, if correct, appearing to me to be of great importance, I was desirous of confirming it by repetition and variations of the experiment.

For this purpose I made use of the calorific galvanometer (as it has been named by M. De la Rive,) instead of the magnetic; and the zinc ball was a little larger than in the preceding experiments, measuring $1\frac{1}{8}$ inch in diameter, and presenting therefore a surface of 3·94 square inches. The sphere was charged as before, and when the ball was placed in the centre

The index advanced	90°
When drawn nearly to the top	90
When thrust nearly to the bottom . . .	87

The index remained perfectly steady during several repetitions of the experiments; and when the connexion with the brass sphere was changed to opposite points. There can be no doubt that these results indicate the same equality of action as before; the slight difference in the lower position being, probably, owing to accidental circumstances, such as the unequal thickness of the membrane at that point, or the speedy saturation of the acid when the solution cannot fall away from the zinc. Thus it would appear, that in none of these positions was there any virtual approximation of the generating and conducting surfaces. The diffusion of the precipitated copper was, however, very much influenced by the position of the ball; when near the top or the bottom, it was thrown down in a compact layer on a segment immediately in its vicinity, and became thinner and thinner over the more remote parts of the sphere. It was only when exactly in the centre that the diffusion was perfectly equal. To observe this effect with the greatest distinctness, the action in each case should not be allowed to continue more than ten or fifteen minutes; for the deposition becomes compact in every part in a longer time, when it is not so easy to judge of the difference of thickness.

My next step was to measure the effects of different portions of the sphere in combination with the zinc ball. I took the lower hemisphere alone of the apparatus and filled it with the solution of copper, and placing a wooden bar across it which supported the membranous bag to carry the acid, I had it in my power to immerse the ball in any required position.

I first placed the same ball as in the last experiment just below the surface exactly in the centre, and the calorific galvanometer indicated 90°: upon removing it within half an inch of the bottom it rose to 115°.

On another occasion I placed the ball in three different positions upon the surface of the liquid in the hemisphere, namely, in the centre and close to each side: in each position the instrument marked 86°. Upon lowering it as close as possible to the bottom it rose to 100°. In all these experiments the precipitated copper was diffused over the whole hemisphere.

It is worthy of remark, that the amount of force thus called into action at the surface of the liquid in the hemisphere, is nearly the same as that from the whole charged sphere, or either of its hemispheres.

These results, I conceive, are not in opposition to the law of radiant forces sug-

gested by the experiments with the entire sphere; although I cannot vouch for the comparative measures being absolutely correct or uninfluenced, in some degree, from day to day by extraneous circumstances, and particularly by changes of temperature: nor can it, indeed, be expected that the experimental deductions from a ball of the dimensions which I employed can do more than approximate to the mathematical demonstrations of the relations of an active point within an attractive sphere.

An accidental circumstance next furnished me with an interesting variation in the combination of the whole sphere. My intention had been to fill it as before, and repeat the experiments; but I afterwards found that the liquid only reached to within 45° from the vertex. The zinc ball which I made use of was $\frac{3}{4}$ inch in diameter, and the membrane was full of the dilute acid. The experiments were made in three different positions of the ball, and with three variations of the wire connecting the circuit with the brass sphere.

The first connexion was made near the top at a point which was not within the contact of the included liquid; the results were

	Caloric Galv.
At the top	45°
Centre	55
Bottom	65

Connexion with the bottom of sphere.

Ball at the top	45°
Ball at the centre	70
Ball at the bottom	77

Connexion with the sphere both at the top and bottom.

Ball at the top	45°
Ball at the centre	73
Ball at the bottom	78

The two last series may be taken to be identical, but I am at a loss to explain the difference of the first series from these two. It is probable that the zinc ball, though wholly immersed in the acid in the membrane, was not below the surface of the exterior solution in the sphere. The results confirm in a general way the conclusions which have been previously suggested.

There was yet another combination which I thought it desirable to try; namely, with the generating ball placed within the charged sphere with its two hemispheres insulated. The zinc ball which I employed for this experiment was one inch diameter, and I tried it first with the standard acid with which all the other experiments had been made, and repeated it with the same acid diluted with an equal bulk of water, in order that I might ascertain whether the law of action were influenced at all by the amount of force put into circulation. The results were perfectly consistent with each other.

	Standard Acid.	Dilute Acid.
	°	°
The zinc ball placed near the bottom, connexion with top insulated hemisphere	52	42
Connected with bottom	59	50
The zinc ball placed near the top, connexion with bottom insulated hemisphere	51	41
Connected with top	59	50
Both hemispheres connected	59	

In these experiments, that hemisphere alone with which the connexion had been made could have influenced the results as a conducting surface.

A question now occurred to me of extreme interest: admitting that the force generated in the preceding combinations where it was limited in its diffusion by the concave surfaces of spherical forms, follows in its action the law of radiant forces, we know that it is not a *simple* radiant force like that of gravity, but a molecular force propagated from particle to particle, and possibly modified by other forces with which the same particles may be endowed. The law of radiation, if established, may, therefore, be the result of the concurrence of more than one force, limited by the spherical combinations with which we have been dealing. The physical law, in ordinary cases, supposes both the sphere and the point within to consist of similar attractive matter; and were the force which we are now investigating of the same nature, it would make no difference whether the sphere consisted of merely conducting metal and a small interior generating ball, or whether the sphere consisted of generating metal and the ball were an inactive conductor,—inactive I mean with regard to the electrolyte. What experimental results, therefore, would the latter combination afford?

I caused two hemispheres of zinc to be made of exactly the same form and dimensions as those of copper, and fitted together in the same way; the interior surfaces of these were thoroughly amalgamated with mercury. When wedged together by their flanges and ring of leather, metallic contact was preserved between the two by a small interior ring of amalgamated zinc. The sphere was charged with dilute acid, and the interior membranous bag with the acid solution of copper. A copper ball of one inch diameter fixed to the end of a varnished wire was then immersed in the latter, and a circuit formed by contact with different parts of the zinc sphere with the intervention of the calorific galvanometer.

The indications of the instrument were as follow:—

	°
Ball at the top	36
Ball at the centre	36
Ball at the bottom	36
Again at the top	35

The index was steady for a short time, but began slowly to decline in each instance for about 10°. Upon agitation of the ball in the solution it always rose to its previous amount.

Upon repeating the experiment and keeping the ball always agitated, the following results were obtained:—

Ball at the top	40°
Ball at the centre	42
Ball at the bottom	42

Another repetition gave

Ball at the top	45
Ball at the centre	46
Ball at the bottom	44

The index always fell when the ball was not agitated, and the decline may probably be ascribed to a change in the saturation of the liquid in immediate contact with the ball, which agitation prevented by keeping the solution in an uniform state.

I now separated the hemisphere and experimented with the lower one alone. The results were as follow:—

Ball at the top	35°
Ball at the bottom	80
Ball at the top	35
Ball half-way between	60

When the ball was placed at the centre of the surface and at the sides almost in contact with the hemisphere, the index rose to 36° in all the three positions.

Hence it appears that the same law was maintained; although the force which circulated was reduced to one half of the amount of that from the first combination.

Before I venture to offer two or three remarks upon this difference, I shall proceed to lay before you the results of some experiments upon combinations of generating and conducting surfaces of other forms, commencing with the cylindrical as approaching in simplicity to the spherical, and for the purpose of connecting my observations with that form which I have found most practically advantageous in the construction of my battery.

I took one of the cells of the small battery*, six inches in height and eleven inches in circumference, charged in the usual way, and found that the single zinc ball of one inch diameter, produced with it a deflection of the magnetic galvanometer of 55°. Two similar balls only increased the deflection to 60°, and its own rod, six inches in length and $\frac{5}{8}$ inch diameter, did not increase the effect. These were exactly the same amounts as were produced by the same generating surfaces in the brass sphere.

An amalgamated zinc cell of exactly the same dimensions, charged with the di-

* Philosophical Transactions, 1836, p. 117.

luted acid and solution of copper in the membrane, with a copper ball one inch diameter, deflected the galvanic needle 45°.

With the calorific galvanometer it moved the index to 25°.

Being desirous of ascertaining whether the same difference between the reversed combinations would exist in a series as in single circuits, I compared together three copper cylinders with three zinc rods, and three zinc cylinders with copper rods, by means of a voltameter. The former produced in $\frac{1}{4}$ hour 3·6 cubic inches of gas. The latter produced in $\frac{1}{4}$ hour 1·75 cubic inches: indicating, as before, that when the generating surface constituted the circumference of the arrangement, the force was only the half of that which was evolved when it formed the centre.

In the copper cylinder it made no difference whether the zinc ball or rod were placed in the centre, or nearly in contact with the side.

I next took an oval copper plate, the diameters of which were $13\frac{1}{2}$ inches by 10 inches, and soldered a copper wire at one extremity of the longest diameter, in a perpendicular position, and placing it in an earthen pan, covered it with a depth of $4\frac{1}{2}$ inches of the acid solution of copper. Over its centre I suspended, by means of a cross bar, a bag of membrane filled with the dilute acid; and nearly at the bottom of this I placed an amalgamated zinc ball, connected with a varnished copper wire. I then formed a circuit by means of the magnetic galvanometer, and the needle was deflected 55°. It remained perfectly steady for half an hour, when the plate was taken out and examined. It was found covered with fresh precipitated copper, the coat being a little thicker at the centre, and becoming thinner by almost insensible gradations towards the edge. At one point it had begun to turn round the edge and to diffuse itself on the under side.

The zinc ball was next placed just below the surface of the liquid in the pan, so that its least distance from the conducting plate was the same as when placed in the centre of the sphere. Upon completing the circuit the needle was deflected 45°.

The zinc ball was now drawn up in the tube so as to remain immersed in the acid, but two inches above the level of the solution; the needle was still deflected 35°. Upon again replacing it below the level it returned to 45°, and fell to the same amount upon once more drawing it up. In this case the lateral diffusion of the efficient force must have been prevented for the first two inches of its course; the mode of its after propagation was the subject of my next inquiry. While the apparatus was in this position, one zinc rod, and afterwards two, connected together, were immersed into the acid, so as to extend through the whole depth of the solution, without deflecting the needle more than 55°.

In these experiments the stem of the copper wire which was soldered to the copper plate was covered with precipitated copper as well as the plate. In subsequent experiments it was protected by varnish.

In order effectually to cut off the lateral radiation from the zinc ball, I placed it in a glass tube, six inches long; and over the lower aperture, which was $1\frac{1}{2}$ inch

diameter, I tied a piece of membrane: the tube was then filled with dilute acid and plunged into the solution of copper contained in the brass hemisphere, against the bottom of which it rested. The ball was supported within half an inch of the diaphragm. Upon closing the circuit by means of the magnetic galvanometer, the needle indicated 40°. At the expiration of five minutes the solution was drawn off and the hemisphere examined: there was found a beautiful well-defined circle of pink copper, two inches diameter, surrounded by a halo of darker colour, evidently of fresh precipitated copper of less thickness, but not extending over more than a fourth part of the surface. From this experiment it was evident that the force had diverged from the aperture of the glass tube, as from a centre, after it had entered the solution; the circle of pink copper being of a diameter half an inch greater than the aperture, and the fainter halo extending some inches around.

I now moved the tube into such a position, that, the zinc ball, remaining where it was in the tube, might be just below the level of the solution in the hemisphere; and now, notwithstanding the greater distance at which it was placed from the conducting surface, the needle rose to 45°, and the precipitated copper made its appearance over the whole hemisphere.

While the zinc ball was in its last position, the glass tube itself was pressed down till it again rested upon the bottom of the hemisphere; all lateral diffusion was thus cut off for a distance of $4\frac{1}{2}$ inches, through which the force was propagated, and it could only spread after it emerged from the tube; the galvanometer indicated 30°. When the glass screen, as it may be called, was again drawn up, the needle returned to 45°.

Similar experiments were often repeated with the substitution of large flat plates for the hemisphere. When the zinc ball was thus confined in a tube with a diaphragm, and placed within an inch of the plate, the precipitation always commenced with a circle a little larger than the aperture, and gradually extended itself, so that after some hours action it formed a circle of four or five inches diameter, and sometimes turned the edge, and made its appearance on the under surface.

Being desirous of ascertaining to what amount the under surface of a plate thus immersed in the electrolyte would affect the action, I covered the upper surface of the oval copper plate with lac varnish, and replaced it in its pan; I then placed the zinc ball in the tube within half an inch of the diaphragm, and plunged it just below the surface of the solution; upon completing the circuit the needle indicated 50°. In ten minutes time I examined the plate, and found the under side covered with beautiful pink copper, with the exception of an irregular oval space, whose diameters were about $4\frac{1}{2}$ inches by 4. The precipitation had evidently begun upon the edge, where it was thickest.

The plate was returned to its position, and the galvanometer again indicated 50°. When a double ball was substituted for the single it rose to 55°. It was again examined after an hour's action, and on the under side presented the appearance of a

Fig. 1.

Fig. 2.

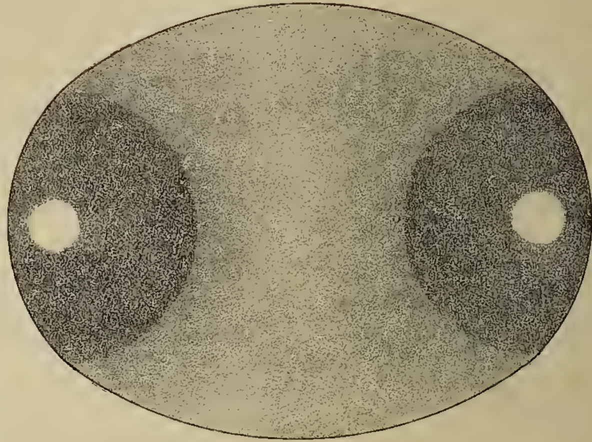


Fig. 3.

Fig. 4.

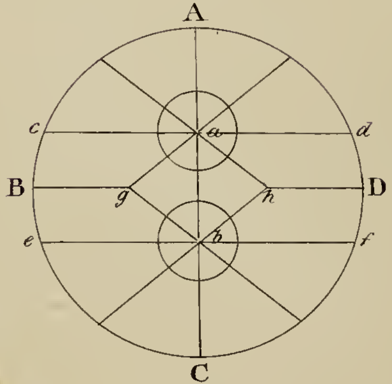
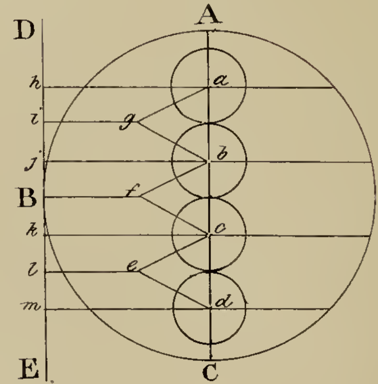


Fig. 5.



J. Basire. lith.

border of compact pink copper, varying from $1\frac{1}{2}$ inch to $\frac{7}{8}$ inch in width, but not wider at the sides than the ends; and the remainder was covered with precipitated copper of a darker red colour, into which the former graduated. On the upper varnished side there were a number of little rosettes of copper, which were loose, giving it a gritty feel, and which evidently proceeded from a number of minute points to which the varnish had not adhered. Fig. 1. (Plate II.) may convey an idea of the appearance of the precipitated copper. Both surfaces of the copper plate having been cleaned, it was replaced in the solution, and the double zinc ball in the membrane, instead of being placed over the centre, was suspended over that end of the oval which was farthest removed from the conducting wire. The galvanometer indicated 48°, and remained perfectly steady for an hour, during which the action was continued. At the expiration of this time the plate was examined, and the pink copper was found deposited only at the extremity under the ball, forming a segment of a circle, of which the ball was the centre, but still not sharply defined, but imperceptibly shading off by a darker colour to the unchanged surface at the other extremity.

Here the efficiency of the generating ball was evidently impeded by the deficiency of the conducting plate on one side. The radiation could only take place towards the other side; and although the conductor was of great extent, the increasing distance of its several points caused it to act at a disadvantage.

After these experiments the plate was cleaned and replaced in the solution; and in order to determine the action of more than one generating point, at a distance from one another upon the same conducting plate, I first placed a single zinc ball in a membrane close to the conducting wire of the plate, and found the deviation of the galvanometer 42°·5. I then broke this connexion, and placed a double ball of zinc over the farthest extremity of the oval; the galvanometer again marked 42°·5. When the circuit was completed with both together the galvanometer only rose to 45°. Upon making or breaking contact with either ball singly, while the other remained in circuit, the needle only varied 2° or 3°. When the positions of the double and single ball were changed, the difference upon breaking the contact with it was 5°. After four hours action the plate was examined, and the pink copper was found deposited in two segments of circles, of which the balls had been the respective centres; and the remainder of the plate was covered with a precipitation of a redder colour. (fig. 2.) This thinner precipitation gave no indications of an increased substance as evidence of additional action at the points where the effects spreading from the two centres must be conceived to have been superimposed.

The plate having been once more cleaned, the double ball was placed nearly over its centre, and when connected with the galvanometer in circuit it affected it 55°. The single ball placed also nearly over the centre, but about two inches removed from the position of the double ball, affected it singly 45°. When the connexion was made with both the deflection increased to 57°. After four hours action with both the balls, a beautiful well-defined oval of pink copper precipitate was found immediately under

the two balls, of the diameter of 4 inches by $3\frac{1}{2}$ nearly, beyond which there was a general diffusion of darker red precipitate (fig. 3.). The surface of this oval was perfectly smooth and compact, and presented no appearance of greater thickness or inequality at the centre than at any other part. The two balls seem to have acted as one oblong piece of generating metal would have done; and, upon the hypothesis of a radiant force from each, no increase was perceived at the points upon which the double set of rays might have been supposed to impinge.

I was desirous to repeat once more the experiments with the opposite sides of the conducting plate, and to connect them more closely with some of the previous series, by measuring the effects upon the calorific galvanometer, and by varying the form of the plate. For this purpose I took a circular plate of copper of the same diameter as the sphere, and immersed it in the copper solution. A zinc ball of $1\frac{1}{8}$ inch diameter, placed in a membrane of acid over the centre, gave the following results:—

	First Series.	Second Series.
Ball at top . . .	79°	82°
Ball at middle . .	89	92
Ball at bottom . .	103	105

After these experiments the copper was found diffused over both sides of the plate, but did not extend to the centre of the under side.

The upper surface of the plate having been thoroughly covered with lac varnish, the experiment was repeated with the following results:—

Ball at top	69°
Ball at middle	75
Ball at bottom	70

The precipitated copper was here found deposited upon the under side in a ring about two inches in breadth from the edge, and there was very little in the centre of the plate, and none upon the upper surface.

I finally covered the under surface with varnish, leaving the upper exposed, with the following results:—

Ball at top	73°
Ball at middle	83
Ball at bottom	93

Hence it appears, that the under surface, which by itself is capable of sustaining an action from the ball in the centre of the solution nearly as great as the upper surface, when combined with the latter, adds no more than 10°, or about one-eighth to its efficiency. It appears also that, whereas with the upper surface, the action increases in some inverse ratio of the distance, of the generating from the conducting surface, with the under surface, there is a maximum point, on both sides of which it decreases.

This point is doubtless dependent upon the angle at which the force, which radiates (as it were) from the ball, meets the edge of the plate.

Wishing now to draw the preceding experiments into closer comparison with similar ones which had hitherto, generally, been made in a different form, I had a square glass cell constructed, measuring $3\frac{1}{2}$ inches and four deep. This was cut in two in the middle, and the edges having been ground admitted of being clamped together, with the interposition of a piece of bladder. When thus put together it formed a cell divided into two by a diaphragm of membrane in the liquid, on either side of which different generating and conducting plates might be immersed. The forms which I selected were copper and zinc plates $3\frac{1}{4}$ inches square, and copper and zinc balls of one inch diameter: the results obtained by different combinations of these, at different distances apart, measured by the calorific galvanometer, are contained in the following Table. The electrolyte which was employed was, in one series, the standard acid on both sides of the diaphragm, and in the other the same acid in contact with the zinc, and the solution of copper with the copper. In the near distance the surfaces were about half an inch apart; in the farther three inches; and the balls were always placed opposite to the centre of the associated plate.

	Equal Plates.		Copper Plate and Zinc Ball.		Zinc Plate and Copper Ball.		Equal Balls.	
	Close.	Distant.	Close.	Distant.	Close.	Distant.	Close.	Distant.
Acid alone	15°	13°	11°	10°	6°	5°	—°	4°
Acid and Sol. Cop.	73	51	67	53	32	30	26	24

The precipitated copper in all these cases had been pretty evenly diffused over the near surface of the plates, and was to be traced to all parts of the opposite surface, but more upon the edges than towards the centres.

Upon this Table we may remark:—

1st. That the energy of the force was about sextupled by the absorption of the hydrogen upon the conducting surface, except in the case of the equal plates, when it was more than quadrupled.

2nd. That the effect of distance was much more decided in the instances where the amount of the circulating force was greater, than in the contrary cases.

3rd. That the amount of force put into circulation from a large surface of zinc towards a central ball of copper was, as in former instances of similar combinations, about one half of that from the reverse arrangement.

4th. That a ball of zinc exposing a surface of 3·14 square inches placed over the centre of a plate of copper exposing on its two sides a surface of twenty-eight square inches, sustained an action of nearly the same amount as a plate of zinc of the same dimensions as the copper, placed at the same distances.

This result, as well as the small effect produced in preceding experiments by substituting two equal balls, or a rod, for one ball of zinc, may, upon the supposition of

a force of the nature of a radiant force, be probably explained by the interference of the rays at points where their directions cross each other.

Let A B C D represent the sphere and *a* and *b* two active points within; the force radiates from *a* to *c* and A and *d*, and from *b* to *e* and *f* and C without interruption. But the rays proceeding from *a* to *g* and from *b* to *g*, encounter one another, and the force would appear to be directed in the diagonal of the two, or from *g* to B, and in a direction parallel to *a c* and *b c*.

A rod may obviously be considered as a succession of such balls or radiant points, and hence the force would be propagated in a direction at right angles to its axis towards a circumscribing sphere or cylinder.

Let *a b c d* represent the radiant points, then will the rays *a g* and *b g* interfere at *g* and pass on to *i*, *b f* and *c f* to B, and *c e*, *d e* to *l*. Or the same letters may represent radiant points in a plate A C, the rays of which will thus pass in a parallel direction to the opposite points of a conducting plate D E.

Without attaching, however, any importance to the geometrical diagrams, I would merely suggest that the resultants of all the radiant points acting from *a* to *d* towards D E may be parallel, and that such an hypothesis would account for the phenomena. The demonstration of this, if possible, would go far beyond my power in mathematical science.

The rays thus supposed to pass between two equal plates become parallel, and hence the decrease of the force will be directly as the distance, as Mr. Snow Harris found it by direct experiment.

Now nearly the whole of the preceding experiments, except those of the zinc sphere, had been made before I had the pleasure of reading the Eleventh of your Series of Experimental Researches on Electric Induction*; and I had been led to the supposition, which I believe I mentioned to you, that the force which is developed by voltaic combinations might be subject to the law of radiant forces; but I had been utterly at a loss to understand how, upon this hypothesis, it could extend its influence to the side of a plate opposite to that to which it was directed in right lines; how, in short (to make use of a term which you have happily employed to describe, what I now believe to be, a perfectly analogous phenomenon), it could "*turn a corner*." Since the perusal of that paper, however, everything seems to me to fall in so naturally with the general views which you have therein explained, that I almost feel as if I were intruding upon ground which is properly your own in venturing to apply the laws which you have established of the "Essential and Fundamental Principle of Induction"† to the explanation of some of the foregoing results. Supposing my views to be correct, you must have been led to them in the natural course of your investigations; and nothing in my own opinion could justify my interference in a work which must have been more completely performed by you, but the circumstance that I was led to it by the obvious direction of my own previous inquiries into voltaic combinations.

* Philosophical Transactions, 1838, p. 1.

† Ibid. p. 2.

The direction of the force through an electrolyte placed between a generating and conducting surface of two metals, may, I conceive, be expressed in the very words which you have employed to describe that of the *direct inductive force* in statical electricity.

"It may be conceived to be exerted in lines between the two limiting conducting surfaces, and is accompanied by a lateral or transverse force equivalent to a dilatation or repulsion of these representative lines; or the attractive force which exists amongst the particles of the electrolyte (dielectric) in the direction of the current (induction) is accompanied by a repulsive or diverging force in the transverse direction*."

The proof of this is exactly of the same nature as that which you have brought forward in the parallel instance of induction, namely, the turning round the corner of a plate; and I cannot but advert to the complete analogy of the case in which you brought your carrier ball near to the middle of a flat disc of metal placed upon an excited shell-lac cylinder when no charge was communicated, although one was obtained at the edge of the disc; and that of the deposition of a ring of precipitated copper round the edge of the under surface of a brass plate while the centre was free from it (fig. 1.).

This "lateral tension of the lines of force on one another" is quite consistent with their *divergence* from an active centre: may it not even be considered as the cause of their radiation? It is most particularly evidenced by the results of those experiments, in which the immediate divergence of the force from the active centre was prevented, by placing the latter in a glass tube, or by drawing it up above the general level of the surrounding electrolyte. In these instances the first impulse must have been propagated in a perpendicular direction; but the instant it was at liberty to influence the general mass, the molecules of the latter were thrown into the polarized state, and the direction of the force opened out as from a centre.

On the other hand, the same "repulsive force in a transverse direction" must be opposed to the *convergence* of the lines from an active sphere towards an interior conducting point, when the force is not stationary but current: may not this opposition account for the reduced action of a sphere of zinc upon a ball of copper? The difference of the *statical induction* and the *current induction* is, that in the former the force is not progressive, while in the latter it is in a state of perpetual flux; the state of polarity, however, and of tension, is maintained in both.

The transfer of the elements of the electrolyte in opposite directions under that peculiar molecular arrangement or polarity, "which is the first step in all electrolyzation," is quite compatible with their unequal distribution upon the limiting conducting surfaces, according to the varying relations of their dimensions and distances, as was evidenced by the unequal precipitation of the reduced copper in several of the preceding instances; but no correspondent inequality of the force can exist upon the surface of the conductors themselves, upon all parts of which it can instantaneously distribute itself with comparative facility.

* Philosophical Transactions, 1838, p. 37.

The principal circumstance which might be supposed to limit the power of an active point within a conducting sphere in any given electrolyte, is the resistance of that electrolyte, which increases in a certain ratio to its depth or thickness; and this thickness may be considered virtually the same, wherever the included point may be placed, but to increase with the diameter of the sphere. It is also the same, and consequently the resistance is also the same, when placed anywhere within the plane which divides the sphere into two hemispheres. But in an insulated hemisphere, the approximation of the active point to the lower surface virtually decreases the thickness of the electrolyte through which its action has to be propagated, by increasing the extent of surface which cuts the divergent lines of force; and consequently the force increases. In this respect the action of a point upon a plate may be considered the same as upon an indefinitely large hemisphere, towards which as the point approaches the force increases.

It appears to me that practical consequences of some importance flow from the preceding conclusions, upon which I may be tempted to address you again at no distant period.

I remain, my dear Faraday,

Ever faithfully yours,

J. F. Daniell.

King's College,
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III. *Of such Ellipsoids consisting of homogeneous matter as are capable of having the resultant of the attraction of the mass upon a particle in the surface, and a centrifugal force caused by revolving about one of the axes, made perpendicular to the surface.*

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1. In the *Conn. des Temps* for 1837 it is announced that a homogeneous ellipsoid with three unequal axes, and consisting of particles that attract one another according to the law of nature, may be in equilibrium when it revolves with a proper velocity about the least axis. Lagrange has considered this problem in its utmost generality. The illustrious Geometer found the true equations from which the solution must be derived: but he inferred from them that a homogeneous planet cannot be in equilibrium unless it have a figure of revolution. Nevertheless M. Jacobi has proved that an equilibrium is possible in some ellipsoids of which the three axes have a certain relation to one another. The same thing is demonstrated by M. Liousville in 23rd cahier of the *Journal de l'École Polytechnique*. M. de Pontécoulant has also touched on the subject*. M. Jacobi has thus detected an inadvertence into which those had fallen who preceded him in this research. He has shown that the equations which, according to Lagrange, are capable of solution only in figures of revolution, may be solved in a certain class of ellipsoids with three unequal axes. But the transcendent equations of M. Jacobi, although fit for numerical computation on particular suppositions, leave unexplored the points of the problem which it is most interesting to know.

It is easy to find a property characteristical of all spheroids with which an equilibrium is possible on the supposition of a centrifugal force. From any point in the surface of the ellipsoid draw a perpendicular to the least axis, and likewise a line at right angles to the surface: if the plane passing through these two lines contain the resultant of the attractions of all the particles of the spheroid upon the point in the surface, the equilibrium will be possible; otherwise not. This will be evident, if it be considered that the resultant of the centrifugal force and the attraction of the mass must be a force perpendicular to the surface of the ellipsoid, which requires that the directions of the three forces shall be contained in one plane. This determination obviously comprehends all spheroids of revolution; but, on account of the

* Tom. iii. Théor. Anal.

complicated nature of the attractive force, it is difficult to deduce from it whether an equilibrium be possible, or not, in spheroids with three unequal axes.

The problem is unconnected with the physical conditions of equilibrium: it is purely a geometrical question respecting a property of certain ellipsoids.

2. Let the three semi-axes of an ellipsoid be represented by

$$k, k\sqrt{1+\lambda^2}, \quad k\sqrt{1+\lambda'^2},$$

λ being supposed greater than λ'; and put x, y, z respectively parallel to the axes, for the coordinates drawn from a point in the surface to the principal sections of the solid: from the same point draw the line ϱ within the ellipsoid at right angles to its surface; and ϱ being limited by the principal section perpendicular to k, the axis of rotation, put p and q for the coordinates of the end of it in that plane, p being parallel to y, and q to z: from the condition that ϱ is perpendicular to the surface of the ellipsoid, it is easy to deduce the values of p and q, viz.

$$p = y \cdot \frac{\lambda^2}{1+\lambda^2}, \quad q = z \cdot \frac{\lambda'^2}{1+\lambda'^2}.$$

Again, from the same point in the surface, draw the line ϱ' in the direction of the resultant of the attraction of the whole mass of the ellipsoid; and let r and s, respectively parallel to y and z, represent the coordinates of the foot of ϱ' in the same principal section as before: then ϱ' will be the diagonal of a parallelopiped of which the three sides are $x, y-r, z-s$; and the only three forces acting parallel to the sides of the parallelopiped and equivalent to the single force in the direction of the diagonal, will be proportional to the sides, $x, y-r, z-s$. Now from the nature of the ellipsoid, the attractive forces perpendicular to the principal sections, are proportional to the coordinates x, y, z; and may be represented by $\mathrm{A}\,x$, $\mathrm{B}\,y$, $\mathrm{C}\,z$: and, as these forces have their resultant in the direction of ϱ', it follows from what has been said, that they will be proportional to $x, y-r, z-s$. In consequence we have these equations,

$$\mathrm{A} = \frac{y}{y-r} \cdot \mathrm{B}, \quad \mathrm{A} = \frac{z}{z-s} \cdot \mathrm{C};$$

$$r = y \,.\left(1 - \frac{\mathrm{B}}{\mathrm{A}}\right), \quad s = z \,.\left(1 - \frac{\mathrm{C}}{\mathrm{A}}\right):$$

and, by combining the values of r and s with those of p and q before found, we obtain

$$\frac{r-p}{s-q} = \frac{y}{z} \cdot \frac{\mathrm{B} - \dfrac{\mathrm{A}}{1+\lambda^2}}{\mathrm{C} - \dfrac{\mathrm{A}}{1+\lambda'^2}}.$$

Let σ denote the third side of the triangle which has ϱ and ϱ' for its other sides: then σ will represent the only force which, together with the attractive force ϱ', will produce a resultant in the direction of ϱ at right angles to the surface of the ellipsoid. Now σ cannot stand for a centrifugal force unless, in every position, it be invariably

parallel to the line $\sqrt{y^2 + z^2}$ drawn from the point in the surface of the ellipsoid at right angles to the axis of rotation; and this condition requires that the triangle of which the sides are σ, $r - p$, $s - q$, shall be similar to the triangle formed by the parallel lines $\sqrt{y^2 + z^2}$, y, z. From the similarity of the triangles, we deduce

$$\frac{r - p}{s - q} = \frac{y}{z}:$$

and hence, in consequence of the last formula, we finally obtain,

$$B - \frac{A}{1 + \lambda^2} = C - \frac{A}{1 + \lambda'^2} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

Every ellipsoid which verifies this formula is capable of an equilibrium when it is made to revolve with a proper angular velocity about the least axis; for the line ϱ' representing the attraction upon a point in the surface, the line σ will represent a centrifugal force, both in quantity and direction; and the resultant of these two forces will be perpendicular to the surface of the ellipsoid.

The equation (1.) results immediately from the investigation of Lagrange, who concluded that it admits of solution only in spheroids of revolution, that is, when $\lambda = \lambda'$ and $B = C$. By expressing the functions A, B, C in elliptic integrals, M. Jacobi has found that the equation may be solved when the three axes have a certain relation. It is therefore demonstrated in general, that a certain class of ellipsoids with three unequal axes is susceptible of an equilibrium on the supposition of a centrifugal force; but it still remains to investigate the precise limits within which this extension of the problem is possible, and to determine the ellipsoid when the centrifugal force is given.

3. In order to solve the problem in the view now taken of it, we must have recourse to the equations of Lagrange, which contain all the necessary conditions. Let f denote the intensity of the centrifugal force at the distance equal to unit from the axis of rotation; the same force urging the point in the surface of the ellipsoid at the distance $\sqrt{y^2 + z^2}$ from the axis, will be equal to $f\sqrt{y^2 + z^2}$, the components of which in the directions of y and z are respectively fy and fz. Now

$$A\,x,\ B\,y,\ C\,z,$$

are the attractions of the mass of the ellipsoid; wherefore the total forces urging the point in the surface are

$$A\,x,\ (B - f)\,y,\ (C - f)\,z.$$

These forces must have their resultant in the direction of ϱ perpendicular to the surface of the ellipsoid; and as they are parallel to the sides of a parallelopiped, of which ϱ is the diagonal, they will be proportional to those sides, that is, to

$$x, \quad y - p = \frac{y}{1 + \lambda^2}, \quad z - q = \frac{z}{1 + \lambda'^2}.$$

We thus obtain these two equations,

$$\frac{B-f}{A} = \frac{1}{1+\lambda^2}, \quad \frac{C-f}{A} = \frac{1}{1+\lambda'^2};$$

and from these we deduce

$$\left.\begin{aligned} f &= B - \frac{A}{1+\lambda^2}, \\ f &= C - \frac{A}{1+\lambda'^2}, \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (2.)$$

which coincide with the equations of LAGRANGE.

It is next requisite to substitute for the symbols A, B, C, what they stand for. The values given in the *Mécanique Céleste* are in a convenient form for this purpose, viz.

$$d\,F = \frac{x^2\,d\,x}{\sqrt{(1+\lambda^2 x^2)\,.\,(1+\lambda'^2 x^2)}},$$

$$A = \frac{3\,M}{k^3}\int_0^1 d\,F, \quad B = \frac{3\,M}{k^3}\int_0^1 \frac{d\,F}{1+\lambda^2 x^2}, \quad C = \frac{3\,M}{k^3}\int_0^1 \frac{d\,F}{1+\lambda'^2 x^2}.$$

In these expressions M is the mass of the ellipsoid; therefore if we put ϱ for the density, we shall have

$$\frac{M}{k^3} = \frac{4\,\pi\,\varrho}{3}\,.\,(1+\lambda^2)^{\frac{1}{2}}\,(1+\lambda'^2)^{\frac{1}{2}}.$$

These several values being substituted in the equations (2.), the result will be

$$\left.\begin{aligned} q &= \frac{f}{\frac{4\,\pi}{3}\,\varrho}, \\ q &= \sqrt{\frac{1+\lambda'^2}{1+\lambda^2}}\,.\int_0^1 \frac{\lambda^2\,.\,3\,x^2\,d\,x\,(1-x^2)}{(1+\lambda^2 x^2)^{\frac{3}{2}}\,(1+\lambda'^2 x^2)^{\frac{1}{2}}}, \\ q &= \sqrt{\frac{1+\lambda^2}{1+\lambda'^2}}\,.\int_0^1 \frac{\lambda'^2\,.\,3\,x^2\,d\,x\,(1-x^2)}{(1+\lambda^2 x^2)^{\frac{1}{2}}\,(1+\lambda'^2 x^2)^{\frac{3}{2}}}. \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (3.)$$

Here q stands for the proportion of the intensities of the centrifugal and attractive forces; it depends only on the kind of matter of which the spheroid is formed, and the velocity of rotation.

4. The equations (3.) comprehend all ellipsoids that are susceptible of an equilibrium on the supposition of a centrifugal force. To begin with the more simple case of the spheroid of revolution, let $\lambda = \lambda' = l$; and the two equations will coincide in one, viz.

$$q = \int_0^1 \frac{l^2\,.\,3\,x^2\,(1-x^2)\,d\,x}{(1+l^2 x^2)^2}, \quad \ldots\ldots\ldots\ldots \quad (4.)$$

which expresses the relation between q and l, in a spheroid of revolution having its semi-axes equal to k and $k\sqrt{1+l^2}$.

From the equation (4.) we learn that q will be known when l is given, or that every spheroid of a determinate form requires an appropriate velocity of rotation.

The inspection of the same equation is sufficient to show that q is positive for all values of l^2; and as it vanishes both when l^2 is zero and infinitely great, it must pass at least once from increasing to decreasing, or it will admit of at least one maximum value. By differentiating with regard to l we obtain

$$\frac{dq}{2\,l\,dl} = \int_0^1 \frac{3\,x^2(1-x^2)(1-l^2x^2)}{(1+l^2x^2)^3}; \quad \ldots\ldots\ldots \quad (5.)$$

from which formula we learn that $\frac{dq}{2\,l\,dl}$ is positive between the limits $l^2 = 0$ and $l^2 = 1$; that it will consist of a positive and a negative part when l^2 is greater than 1; and the positive part decreasing while the negative part increases, that it will ultimately be negative when l^2 is infinitely great. It follows therefore that $\frac{dq}{2\,l\,dl}$ can be only once equal to zero, and consequently that q can have only one maximum value, while l^2 increases from 0 to ∞. Applying to the equations (4.) and (5.) the known method of integration, we get

$$q = \frac{3(3+l^2)}{2\,l^3}\,\text{arc tan}\,l - \frac{9}{2\,l^2},$$

$$\frac{dq}{2\,l\,dl} = \frac{3(9+l^2)}{2\,l^4}\,\text{arc tan}\,l - \frac{3\,l\,(9+7\,l^2)}{1+l^2},$$

of which expressions the first will verify the other. To determine the maximum of q, we have

$$\frac{dq}{2\,l\,dl} = 0$$

$$\text{arc tan}\,l = \frac{9\,l + 7\,l^3}{(1+l^2)(9+l^2)};$$

and the only value of l in this last equation is

$$l = 2{\cdot}5293.$$

By substituting this value of l we obtain 0·3370 for the maximum of q. With respect to spheroids of revolution it thus appears that an equilibrium is impossible when q or $\frac{f}{\frac{4\pi}{3}\rho}$ is greater than 0·3370: in the extreme case, when q is equal to 0·3370, there is only one form of equilibrium, the axes of the spheroid being

$$k \text{ and } k\sqrt{1+(2{\cdot}5293)^2} = 2{\cdot}7197\,k;$$

but when q is less than 0·3370 there are two different forms of equilibrium, the equatorial radius of one being less, and of the other greater, than 2·7197 k, k being the semi-axis of rotation.

The number of the forms of equilibrium in spheroids of revolution is purely a mathematical deduction from the expression of q; and as this has been known since the time of Maclaurin, the discussion of it was all that was wanted for perfecting this part of the theory.

5. Returning now to the general equations of the problem, let

$$\varphi = \int_0^1 3\,x^2\,d\,x\,.\,\frac{\sqrt{(1+\lambda^2)\,.\,(1+\lambda'^2)}}{\sqrt{(1+\lambda^2 x^2)\,.\,(1+\lambda'^2 x^2)}};$$

and it will be found that the two equations (3.) are thus expressed:

$$q = \frac{d\,\varphi}{d\,\lambda}\,\lambda,\quad q = \frac{d\,\varphi}{d\,\lambda'}\,\lambda'.$$

Further, put $p = \lambda\,\lambda'$, $\tau^2 = (\lambda - \lambda')^2$, and we shall have

$$\varphi = \int_0^1 3\,x^2\,d\,x\,.\,\frac{\sqrt{(1+p)^2+\tau^2}}{\sqrt{(1+p\,x^2)^2+\tau^2\,x^2}};$$

and the two values of q in the partial differentials of φ relatively to λ and λ' being expressed in the partial differentials relatively to p and τ^2, we shall obtain

$$q = \frac{d\,\varphi}{d\,\lambda}\,\lambda = \frac{d\,\varphi}{d\,p}\,p + \frac{d\,\varphi}{\tau\,d\,\tau}\,(\lambda - \lambda')\,\lambda,$$

$$q = \frac{d\,\varphi}{d\,\lambda'}\,\lambda' = \frac{d\,\varphi}{d\,p}\,p - \frac{d\,\varphi}{\tau\,d\,\tau}\,(\lambda - \lambda')\,\lambda'.$$

These two values of q coalesce in one when $\lambda - \lambda' = 0$, that is, in spheroids of revolution; and we thus fall again upon the same equation that has already been discussed. In all other cases the two values cannot subsist together, unless

$$\left.\begin{aligned} q &= \frac{d\,\varphi}{d\,p}\,p, \\ 0 &= \frac{d\,\varphi}{\tau\,d\,\tau}, \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (6.)$$

which equations apply exclusively to ellipsoids with three unequal axes, and solve the problem with regard to that class. The latter of the equations (6.) expresses the relation that the two quantities p and τ^2 must have to one another in every ellipsoid with three unequal axes which is susceptible of an equilibrium. The fluxional operation indicated being performed in the same equation, the result will be,

$$0 = \int_0^1 \frac{x^2\,(1-x^2)\,(1-p^2\,x^2)\,d\,x}{\left((1+p\,x^2)^2+\tau^2\,x^2\right)^{\frac{3}{2}}}, \quad \ldots\ldots\ldots \quad (7.)$$

which is no other than a transformation of the equation (1.), and is equivalent to other transformations of the same equation found by M. Jacobi and M. Liousville.

The formula (7.) cannot be verified unless p, or $\lambda\,\lambda'$, be greater than 1; for if p were equal to 1, or less than 1, the integral would be positive. This agrees with the limitation of M. Jacobi.

If any value be assigned to τ^2, it is evident that a corresponding value of p may be found which will verify the formula (7.): for, if p be made to increase continually above 1, the integral, which is positive at first, will finally be negative; and it must be zero, in passing from one of these states to the other. This proves that there does exist an infinite number of ellipsoids not of revolution, which are susceptible of an equilibrium.

Let V stand for the integral in the equation (7.); and supposing that p and τ^2 vary so as always to satisfy that equation, we shall have

$$\frac{dV}{dp}\,dp + \frac{dV}{\tau\,d\tau}\,\tau\,d\tau = 0.$$

Now, τ^2 representing any positive quantity, we may conceive it to increase from zero to be infinitely great; in which case it follows from the nature of the function V, that during the whole increase $\frac{dV}{\tau\,d\tau}\,\tau\,d\tau$ will be negative: wherefore the other term $\frac{dV}{dp}\,dp$ will be positive; which requires that p decrease continually. Since p decreases when τ^2 increases, the greatest value of p will answer to the least value of τ^2, that is, to zero; and hence, by making $\tau^2 = 0$ in the formula (7.), we shall obtain this equation, viz.

$$0 = \int_0^1 \frac{x^2(1-x^2)(1-p^2x^2)\,dx}{(1+p\,x^2)^3},$$

for finding the greatest value of p.

It is obvious that there is only one value of p that will verify the equation just found; for the integral can pass only once from being positive to be negative while p increases from 1 to be infinitely great. Let $p = l^2$, $l\,x = z$; and the equation will be changed into this which follows,

$$0 = \int_0^l \frac{dz\left(l^2z^3 - (1+l^4)\,z^4 + l^2z^6\right)}{(1+z^2)^3}:$$

of which the integral is,

$$0 = l^2 z + \frac{(1+l^2)^2}{8}\cdot\frac{3z+5z^3}{(1+z^2)^2} - \frac{3+14l^2+3l^4}{8}\int_0^l \frac{dz}{1+z^2}:$$

and hence, by making $z = l$, we deduce

$$\text{arc tan } l = \frac{3l + 13l^3}{3 + 14l^2 + 3l^4}.$$

The only solution of this equation is $l = 1{\cdot}3934$; and $1{\cdot}9414$ is therefore the greatest value of $p = l^2$. Thus, in all the ellipsoids susceptible of an equilibrium by revolving about the least axis, $\lambda\lambda' = p$ is contained between the limits $1{\cdot}9414$ and 1, while $(\lambda - \lambda')^2 = \tau^2$, increases from zero to be infinitely great.

An elliptical spheroid formed of a homogeneous fluid, can be in equilibrium by the action of a centrifugal force, only when it revolves about the least axis. What has been said determines completely the series of ellipsoids with which an equilibrium is possible, when the three axes are unequal. Representing these axes by

$$k,\quad k\sqrt{1+\lambda^2},\quad k\sqrt{1+\lambda'^2},$$

it has been shown that $\lambda\lambda'$ must be contained between the limits $1{\cdot}9414$ and 1, while $(\lambda - \lambda')^2$ varies from zero to be infinitely great. One limit is when $\lambda = \lambda'$, being a spheroid of revolution of which the axes are

$$k \text{ and } k\sqrt{2{\cdot}9414} = k \times 1{\cdot}7150.$$

Supposing λ and λ' to vary from this extreme, when the first increases, the other will decrease; so that, when λ is infinitely great, λ' will be zero; which proves that the other extreme limit is a cylinder extending indefinitely on either side of the base, which is a circle having k for its radius.

6. It remains to consider the value of q. In the first of the equations (6.), let the operation indicated be performed, and the result will be

$$q = \int_0^1 \frac{p \,.\, 3x^2(1-x^2)\,dx \,.\, \{(1+p)(1+px^2) + p\tau^2x^2\}}{\left((1+p)^2+\tau^2\right)^{\frac{1}{2}} \,.\, \left((1+px^2)^2+\tau^2x^2\right)^{\frac{3}{2}}};$$

and from this we obtain the value of q in the extreme case when $\tau^2 = 0$, or when λ and λ' are equal, viz.

$$q = \int_0^1 \frac{p \,.\, 3x^2(1-x^2)\,dx}{(1+px^2)^2},$$

which is no other than the determination of q in a spheroid of revolution having its axes equal to

$$k \text{ and } k\sqrt{2{\cdot}9414} = k \times 1{\cdot}7150.$$

In the other extreme case, when τ^2 is infinitely great, q is zero.

It has been shown that for every given value of τ^2, there is only one value of p, and only one ellipsoid; and when τ^2 and p are both ascertained, the foregoing expression proves that q is fully determined. Thus there is an appropriate value of q to every ellipsoid susceptible of an equilibrium.

In the formula for q, one of the two quantities, τ^2 and p, increases when the other decreases; and hence it may be surmised that more than one ellipsoid may answer to a given value of q. Some calculation is necessary to elucidate this point. For the sake of abridging expressions, put

$$\begin{aligned} \mathrm{P} &= \sqrt{(1+p)^2+\tau^2} \\ \mathrm{Q} &= \sqrt{(1+px^2)^2+\tau^2x^2} \\ \mathrm{M} &= p(1+p)(1+px^2) + p^2\tau^2x^2 \\ du &= 3x^2(1-x^2)\,dx, \end{aligned}$$

the variation of du being between the limits $x = 0$ and $x = 1$: then, the foregoing value of q will be thus written:

$$q = \int \frac{du \,.\, \mathrm{M}}{\mathrm{P}\,\mathrm{Q}^3};$$

and, q being considered a function of p and τ^2, the fluxion with respect to p will be

$$\frac{dq}{dp} = \int \frac{du}{\mathrm{P}\,\mathrm{Q}^3} \,.\, \left\{\frac{d\mathrm{M}}{dp} - \frac{(1+p)\mathrm{M}}{\mathrm{P}^2} - \frac{3x^2(1+px^2)\mathrm{M}}{\mathrm{Q}^2}\right\}:$$

it will be found that

$$\frac{(1+p)\mathrm{M}}{\mathrm{P}^2} = p(1+px^2) - \frac{\tau^2p(1-p^2x^2)}{\mathrm{P}^2},$$

$$\frac{3x^2(1+px^2)\mathrm{M}}{\mathrm{Q}^2} = 3px^2(1+p) - \frac{3\tau^2px^4(1-p^2x^2)}{\mathrm{Q}^2};$$

wherefore

$$\frac{d\,q}{d\,p} = \left(\frac{1}{\text{P}} + \frac{\tau^2 p}{\text{P}^3}\right) . \int \frac{d\,u\,(1 - p^2 x^2)}{\text{Q}^3} + \frac{p}{\text{P}} \int d\,u\,(1 - x^2)$$

$$+ \frac{p\,\tau^2}{\text{P}} . \int d\,u\,.\,\frac{2\,\text{Q}^2 x^2 + 3\,x^4 - 3\,p^2 x^6}{\text{Q}^5}\,;$$

in consequence of the formula (7.) the first term is zero, so that we have

$$\frac{d\,q}{d\,p} = \frac{p}{\text{P}} \int d\,u\,(1 - x^2) + \frac{p\,\tau^2}{\text{P}} \int d\,u\,.\,\frac{2\,x^2 + (3 + 4\,p)\,x^4 - p^2 x^6 + 2\,\tau^2 x^4}{\text{Q}^5}.$$

And because $3 + 4\,p$ is always greater than p^2, it follows that $\frac{d\,q}{d\,p}$ is essentially positive.

Again, by taking the fluxion relatively to τ^2, we have

$$\frac{d\,q}{\tau\,d\,\tau} = \int \frac{d\,u}{\text{P}\,\text{Q}^3} . \left\{ \frac{p^2 x^2\,\text{P}^2 - \text{M}}{\text{P}^2} + \frac{p^2 x^2\,\text{Q}^2 - 3\,x^2\,\text{M}}{\text{Q}^2} \right\};$$

that is,

$$\frac{d\,q}{\tau\,d\,\tau} = - \frac{p + p^2}{\text{P}^3} \int d\,u\,.\,\frac{1 - p^2 x^2}{\text{Q}^3}$$

$$- \frac{p}{\text{P}} \int d\,u\,.\,\frac{(3 + 2\,p)\,x^2 + (3\,p + p^2)\,x^4 - p^3 x^6 + 2\,p\,\tau^2 x^2}{\text{Q}^5}:$$

Of this value the first term is zero by the formula (7.); and attending to the limits of p and of the integral, the second term is essentially negative.

Now we have

$$d\,q = \frac{d\,q}{d\,p} . d\,p + \frac{d\,q}{\tau\,d\,\tau} . \tau\,d\,\tau:$$

if we suppose τ^2 to increase, p will decrease; and according to what has been shown the two parts of $d\,q$ will be negative. Wherefore, while τ^2 increases from zero to be infinitely great, q will decrease continually from its first value to zero; and for every possible value of q there will be only one value of τ^2, and consequently only one ellipsoid susceptible of an equilibrium.

It would be superfluous to pursue this investigation further, and a mere waste of labour to seek the easiest formulas for solving a problem which, it appears from what has been shown, can have no application in the theory of the figure of the planets. It is extremely probable that no such figures as those required for the equilibrium of ellipsoids with three unequal axes, will be found to exist in nature. It seems difficult to admit that any circumstances, or the action of any forces we are acquainted with, could induce upon a mass of fluid a figure adjusted with such mathematical nicety to the attraction of the mass and the centrifugal force. If the existence of such a figure can be supposed, would it be permanent? Would not the least action of the other bodies of the system upon it be sufficient to destroy the exact confor-

mation on which the equilibrium depends, and leave the fluid to adjust its figure solely by the attraction and the centrifugal force of its particles? The discovery of Jacobi makes no change in the usual theory of the figure of the planets; but it is valuable, as it completes a mathematical speculation, and finally settles what relates to the figure of ellipsoids susceptible of an equilibrium.

IV. *Researches towards establishing a Theory of the Dispersion of Light. No. IV.*

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Introductory Remarks.

In my last communication I laid before the Royal Society a comparison of the results of observation and of theory, with respect to the dispersion of light, in the instances of the refractive indices for the standard rays in fifteen different cases of transparent media (some being the same medium at different temperatures), including those which exhibit the greatest range, and the highest numbers, of any yet subjected to this kind of observation. The agreement with the theory was found to be sufficiently close for the lower cases, but displayed an increasing discrepancy as we advanced towards the higher. The theoretical formula employed was one derived from the undulatory hypothesis, by a process involving some limitations, which rendered it only approximative; and, in conclusion, I remarked that by pursuing the investigation to a greater degree of development, or by adopting methods of a more precise character, it was still reasonably to be hoped that a more close coincidence might be found.

I alluded specifically to the methods of M. Cauchy and of Mr. Kelland, as those to which we might look for the means of following up the inquiry with good prospect of success. Of the former (delivered in the Nouveaux Exercices de Mathématiques, Prague, 1835–6, and extending through livraisons 1 to 8 inclusive), I can only say that the investigations are of so extremely elaborate a character, that I was glad, in the first instance at least, to try any other method which might seem to promise results without involving calculations of such overwhelming extent as those by which the distinguished author establishes the exact agreement with theory of all the indices observed by Fraunhofer.

I therefore commenced with a trial of the method proposed in the memoir of Mr. Kelland*, applying it of course in the first instance to the case of the most highly dispersive substance, oil of cassia, in which the greatest discrepancy had before appeared. Owing to an obscurity in the statement of an important part of the process in the paper referred to, I was led to communicate with the author, and

* Cambridge Transactions, vol. vi. Part I.

soon received from him a statement of the results of theory for oil of cassia, in which the discrepancies were almost wholly removed.

I have since verified that calculation, and have performed similar computations for the only other cases in which material differences before appeared.

The object of the present communication is to state these results, with the necessary data of the calculations; and further, to elucidate the general method, so as to render it more readily applicable to other cases which may arise in the further prosecution of the determination of refractive indices; and to notice the present condition in which the theory may be considered to stand with respect to this material portion of its experimental evidence.

Explanation of the Formula.

The formula adopted in my preceding papers includes essentially the development of the term

$$\frac{\sin\left(\frac{\theta}{\lambda}\right)}{\left(\frac{\theta}{\lambda}\right)}.$$

This of course gives a series involving the *even* powers of λ with certain coefficients. And the practical differences in the *methods of calculation* turn entirely on the number of terms to which it may be thought necessary to pursue this series, or the mode of finding or eliminating the coefficients.

As it does not enter into my present design to refer to the *physical* principles of the theory, I will merely here observe, that though such principles have been assumed under some difference of aspect by the several eminent mathematicians who have treated the subject, yet the formulas deduced for the dispersion have, in every instance, resulted the same as far as the *form* of the series is concerned, differing only in regard to the nature of the summation and the coefficients involved.

As it is in regard to the numerical comparison with experiment that I am at present engaged in considering the subject, I have been chiefly interested in comparing these methods so far as to see whether, when one might fail in giving sufficiently close coincidences, another might cause the discrepancies to diminish or disappear.

In this view then, referring to Mr. KELLAND's method, it may be necessary for its better elucidation to state it *generally* as follows. Supposing it sufficient to take three terms of the series, the relation of the refractive index (μ) to the wave-length *in* the medium ($\lambda_{\prime}$) may be expressed thus:

$$\frac{1}{\mu^2} = p - \frac{1}{\lambda_{\prime}^2} q + \frac{1}{\lambda_{\prime}^4} l.$$

Our comparison, however, is to be made with the wave-length in air or vacuum, which, in order to express that in the medium, must be reduced in the ratio of the refraction for the medium and for the ray, (which is not expressed in the author's formulas,) or, λ being the wave-length in air, we must take

$$\lambda_{\prime} = \frac{\lambda}{\mu},$$

and the formula becomes

$$\frac{1}{\mu^2} = p - \left(\frac{\mu}{\lambda}\right)^2 q + \left(\frac{\mu}{\lambda}\right)^4 l$$

taking such formulas successively for the different standard rays, between any two, as those for B and E, the constant p is eliminated: and combining these with a third, as that for H, the coefficients q and l are determined. For brevity writing

$$\frac{1}{\mu^2_{B}} = b, \quad \left(\frac{\mu}{\lambda}\right)^2_B = \beta, \quad \left(\frac{\mu}{\lambda}\right)^4_B = \beta^2;$$

and similarly expressing by $e, \varepsilon, \varepsilon^2$; h, η, η^2; the corresponding quantities for the rays E and H, we shall have

$$(b - e) = (\varepsilon - \beta)\, q - (\varepsilon^2 - \beta^2)\, l,$$
$$(e - h) = (\eta - \varepsilon)\, q - (\eta^2 - \varepsilon^2)\, l;$$

whence we obtain,

$$l = \frac{(\varepsilon - \beta)(e - h) - (\eta - \varepsilon)(b - e)}{(\varepsilon - \beta)(\eta^2 - \varepsilon^2) - (\eta - \varepsilon)(\varepsilon^2 - \beta^2)},$$

$$q = \frac{(b - e) + (\varepsilon^2 - \beta^2)\, l}{(\varepsilon - \beta)}.$$

Knowing the values of λ from the determinations of FRAUNHOFER, it becomes easy in the above formula to introduce the values of $\left(\frac{\mu}{\lambda}\right)$ taking the indices as given by observation for the particular medium: we, thus, first determine the constants q and l for the medium, and having done this, by the aid of these combined again with the indices given by observation a value of p is deduced for each ray by the formula,

$$p = \frac{1}{\mu^2} + \left(\frac{\mu}{\lambda}\right)^2 q - \left(\frac{\mu}{\lambda}\right)^4 l;$$

and if these values of p for the different rays result *equal*, the theory is verified.

Mr. KELLAND has thus verified it to a degree of accuracy, which will probably be deemed sufficient, for all the indices determined by FRAUNHOFER.

The following Table contains the logarithms of the values of $\frac{1}{\lambda^2}$ for the standard rays after the determinations of FRAUNHOFER, without their index.

Ray	$\log \frac{1}{\lambda^2}$.
B	·18999
C	·23165
D	·32508
E	·42216
F	·48236
G	·59885
H	·66892

In order to simplify the numerical calculations, it is found convenient to regard the two last terms of the formula as involving factors which are respectively some power of ten in the numerators, to the same amount as the number of places which would be found in the values of λ^2 and λ^4 in the denominators.

In applying the theory to the cases of particular media, we have to combine the values in the above Table with those of the indices obtained from observation. These I have taken from my own approximate determinations, as originally given in a separate memoir, and quoted in my last paper in the Philosophical Transactions, 1837, Part I.

In the following cases therefore the logarithms of λ^2 are taken as above, and after deriving those of $\left(\frac{\mu}{\lambda}\right)^2$ and of $\left(\frac{\mu}{\lambda}\right)^4$ a common index 4 is added: from these we obtain in the first instance the values of q and l, and thence again those of p for each ray.

I have not, in the present instance, thought it necessary to go through these somewhat laborious calculations for more than those three cases which in my former investigations appeared to present the greatest discrepancies with theory, viz. the oil of cassia, which gave the greatest discordances; and the two sets of observations on sulphuret of carbon at the respective temperatures of 12° and 22° centigrade.

Comparison of observed refractive indices with the results of Mr. Kelland's *theory.*

I. Sulphuret of Carbon. Temp. 12°.

Ray.	Log. μ^2 from obs.	Values of $\frac{1}{\mu^2}$ from obs.	$\left(\frac{\mu}{\lambda}\right)^2 q.$	$\left(\frac{\mu}{\lambda}\right)^4 l.$	$p.$
B	·42084	·37946	·01461	·00031	·39438
C	·42214	·37832	·01612	·00037	·39481
D	·42804	·37322	·02027	·00059	·39408
E	·43476	·36749	·02573	·00096	·39418
F	·44106	·36220	·03000	·00131	·39351
G	·45392	·35163	·04041	·00237	·39441
H	·46614	·34187	·04884	·00352	·39423

$$\log l = \bar{9}{\cdot}27073$$
$$\log q = \bar{7}{\cdot}55372$$

II. Sulphuret of Carbon. Temp. 22°.

Ray.	Log. μ^2 from obs.	Values of $\frac{1}{\mu^2}$ from obs.	$\left(\frac{\mu}{\lambda}\right)^2 q.$	$\left(\frac{\mu}{\lambda}\right)^4 l.$	$p.$
B	·41408	·38470	·01784	·00029	·40225
C	·41774	·38217	·01979	·00036	·40160
D	·42288	·37768	·02484	·00057	·40195
E	·42996	·37157	·03157	·00090	·40224
F	·43608	·36637	·03678	·00125	·40190
G	·44878	·35588	·04953	·00227	·40314
H	·46136	·34565	·05922	·00332	·40225

$$\log (-l) = \bar{9}{\cdot}26050$$
$$\log q = \bar{7}{\cdot}64725$$

III. Oil of Cassia.

Ray.	Log μ^2 from obs.	Values of $\frac{1}{\mu^2}$ from obs.	$\left(\frac{\mu}{\lambda}\right)^2 q$.	$\left(\frac{\mu}{\lambda}\right)^4 l$.	p.
B	·40197	·39630	·01163	·00200	·40993
C	·40378	·39466	·01285	·00245	·40996
D	·40916	·38980	·01614	·00386	·40980
E	·41661	·38316	·02053	·00624	·40993
F	·42411	·37661	·02399	·00852	·40912
G	·44058	·36259	·03259	·01572	·41090
H	·46100	·34594	·04013	·02384	·40991

$\log l = \bar{8}{\cdot}11760$

$\log q = \bar{7}{\cdot}47363$

Observations on the above Results.

In the case of oil of cassia the accordance in the values of p appears sufficiently close; especially considering that the experimental data can only be regarded as approximations, as fully appears from my paper on the determination of the indices. The only material discrepancy is in the ray G; and it is this ray for which Mr. Kelland himself has always found theory in excess in the calculation of Fraunhofer's indices, and has made some remarks on the point in his memoir. Upon the whole, considering this as the extreme case as yet known and examined, the superiority of Mr. Kelland's method will be sufficiently manifest; and it will be allowed that this extreme case has been thus brought as far at least within the limits of accordance as we can perhaps reasonably expect in the present state of our means of investigation.

The case of sulphuret of carbon at the temperature of 12° is also brought into very satisfactory agreement with theory by the present method.

The other case of the same substance at the temperature of 22° still exhibits some discordance. The ray G is here again in excess; but the differences follow no regular order, being sometimes in excess, sometimes in defect. This at least shows that although the series is not rapidly convergent, in this case the addition of another term would not remove the discordance.

With regard to the error which is always found so marked in the ray G, Mr. Kelland in a letter to me, observed that in that ray it would seem reasonable to entertain some suspicion as to the experimental data. Now there is one circumstance which may corroborate such suspicion. The determinations of the values of λ, as is well known, were made from the interference-spectrum, in which the blue end, with its dark lines, is most *contracted*. In the refraction-spectra, (and more so in the more dispersed,) it is the most *expanded*: and the dark bands which in the lower cases appear single, in the higher are *resolved* into several lines, in some instances separated by very sensible intervals: and this difference must be still more marked in comparing the highly dispersed spectra with that of interference. The ray G, in par-

ticular, is thus resolved into an assemblage of small lines. Thus some uncertainty may be fairly admitted to exist in the data; at any rate enough to render further examination desirable before we can pronounce on the insufficiency of the theory.

General Remarks on the Formula.

If the accordances be allowed to come sufficiently within the limits of error, it may not be improper to add a remark with respect to the entire nature of the formula, and the light in which, (in its present state,) the theory of dispersion must be regarded.

The relation here expressed between the index and the wave-length involves three constants dependent on the medium; which must be in some way derived from experimental data: and which are here directly deduced by assuming some three, at least, of the observed refractive indices for the medium.

The whole process then seems equivalent to assuming these three indices, and then interpolating the intermediate values. This, though under a different form, is also palpably the case with the method adopted in my former paper.

Now it may be contended that this actually carries us but a very little way towards a real or satisfactory explanation, and that a complete theory ought to assign also an independent relation between the constants.

The consideration of this point has been included in the valuable researches lately made by Professor Lloyd of Dublin, given in a paper read before the Royal Irish Academy, and noticed in the reports of that body, (Nos. 2 and 3.). But I have been informed by the author that, in pursuing that research, he has found theory, as yet, incapable of furnishing the relation in question.

It seems, therefore, that in the present state of our knowledge we must be content to regard the *constants* of the formula as unexplained by theory. But the process by which we here obtain them, (viz. by assuming three indices from observation,) may be viewed as simply *auxiliary*. The main calculation may be regarded as independent, and considered to involve two of these constants only *as if* they had been adopted *empirically*; whence we proceed to verify the formula by the coincidences of the values of the third, viz. p. But even with this deficiency, it seems to me not an unimportant step to be able, with two empirical constants, dependent on the medium, but independent of the ray, to assign a third quantity, which expresses for each ray a relation between the wave-length and the refractive index, with so near an approximation to the truth, even in the most extreme case as yet known.

Oxford, January 7, 1838.

V. *On the Colours of Mixed Plates.* *By Sir* David Brewster, *K.G.H. F.R.S.*

Received October 25,—Read December 14, 1837.

The colours of mixed plates were discovered by Dr. Thomas Young*, and described in the Philosophical Transactions for 1802. He produced them by interposing small portions of water, or butter, or tallow between two plates of glass, or two object glasses pressed together so as to give the ordinary colours of thin plates. In this way portions or cavities of air were surrounded with water, butter, or tallow; and on looking through this combination of media he saw fringes or rings of colour six times larger than those of thin plates that would have been produced had air alone been interposed between the glasses. These fringes or rings of colour were seen by the direct light of a candle, and began from a white centre like those produced by transmission; but on the dark space next the edge of the plate, Dr. Young observed another set of fringes or rings, complementary to the first, and beginning from a black centre like those produced by reflection. This last set of colours was always brighter than the first.

The following is Dr. Young's explanation of these two series of colours.

"In order to understand," says he, "this circumstance, we must consider that where a dark object is placed behind the glasses, the whole of the light which comes to the eye is either refracted through the edges of the drops, or reflected from the internal surface; while the light which passes through those parts which are on the side opposite to the dark object consists of rays refracted as before through the edges, or simply passing through the fluid. The respective combinations of these portions of light exhibit a series of colours of different orders, since the internal reflection modifies the interference of the rays on the dark side of the object, in the same manner as in the common colours of thin plates seen by reflection. When no dark object is near, both these series of colours are produced at once; and since they are always of an opposite nature at any given thickness of a plate, they neutralize each other and constitute white light†."

In so far as I know, these observations have not been repeated by any other philosopher; and subsequent authors have only copied Dr. Young's description of the phenomena and acquiesced in his explanation of them. In taking up this subject I

* Since this paper was written I find that this class of colours was discovered by M. Mazeas, and that his experiments were repeated and varied by M. Dutour.

† Philosophical Transactions, 1802. Dr. Young republished the same explanation of mixed plates in 1807 in his Elements of Natural Philosophy. See vol. i. p. 470, 787; vol. ii. 635, 680.

never doubted the accuracy or the generality of the results obtained by so distinguished a philosopher. I was induced to study the phenomena of mixed plates as auxiliary to a more general inquiry; and having observed new phenomena of colour in mineral bodies, which have the same origin as those of mixed plates, and which lead to conclusions different from those of Dr. Young, I am anxious that they should be described in the same work which contains his original observations.

Having experienced considerable difficulty in obtaining satisfactory specimens of the colours of mixed plates by using the substances employed by Dr. Young, I sought for a method of producing them which should be at once easy and infallible in its effects. With this view I tried transparent soap, and whipped cream, which gave tolerably good results; but I obtained the best effect by using the white of an egg beat up into froth. To obtain a proper film of this substance I place a small quantity between the two glasses, and having pressed it out into a film I separate the glasses, and by holding them near the fire I drive off a little of the superfluous moisture. The two glasses are again placed in contact, and when pressed together so as to produce the coloured fringes or rings, they are then kept in their place either by screws or by wax, and may be preserved for any length of time.

If we now examine with a magnifier of small power the thin film of albumen, we shall find that it contains thousands of cavities exactly resembling the strata of cavities which I have described as occurring in topaz, quartz, sulphate of lime and other minerals*; and if we look through the film at the margin of the flame of a candle, we shall perceive the two sets of colours described by Dr. Young, the one upon the luminous edge of the flame, and the other on the dark space contiguous to it. The first we shall call the *direct*, and the second, which are always the brightest, the *complementary fringes*.

If we apply a higher magnifying power to the albuminous films, and bring the edge of one of the cavities to the margin of the flame, we shall perceive that both the *direct* and the *complementary* colours are formed at the very edge, the complementary ones appearing just when the direct ones have disappeared, by the withdrawal of the edge from the flame.

As the colours therefore are produced solely by the edges of the cavities, their intensity must, *cæteris paribus*, depend on the smallness of the cavities, or the number of edges which occur in a given space. When we succeed in forming an uniform film in which the cavities are like a number of minute points, the phenomena are peculiarly splendid and we are enabled to study them with greater facility. When the edges of these cavities are seen by an achromatic microscope, and in direct light, neither the direct nor the complementary colours are visible; but if we gradually withdraw the lens from the cavities a series of beautiful phenomena appear. When the vision first becomes indistinct both the direct and the complementary colours appear at the same time, specks of the *complementary red* alternating with brighter

* Edin. Trans. vol. x. Part I. 407.

specks of the *direct green* light. By increasing the distance of the lens from the cavities, the complementary specks become less and less visible, and we see only the direct green light.

In order to study these phenomena by observing the action of a *single* edge upon light, and to ascertain the effect of an edge when there were no prismatic edges to refract, and no internal surface to reflect light, I conceived the idea of immersing thin plates of a solid substance in a fluid of such a refractive power, that the thickness of the plates should be virtually reduced to the same degree of thinness as the film of albumen between the plates of glass. The new substance described by Mr. Horner*, and which I shall call *nacrite*, furnished me with the means of performing this experiment. I accordingly inclosed the thinnest films of it between two plates of glass containing balsam of capivi; and I had the satisfaction of observing that the bounding edge of the plate and the fluid produced the identical direct and complementary colours above described.

The bounding edge which I selected for observation gave a *bright green* for the *direct*, and a *bright red* for the *complementary* tint. This edge appeared as a narrow distinct black line, exceedingly well defined, and of a uniform breadth like the finest micrometer wire. It consequently obstructed the incident light and produced the phenomena of diffracted fringes. These fringes, however, were modified by the peculiar circumstances under which they were produced, and exhibited in their tints both the direct and complementary colours under consideration.

When the diffracted fringes are viewed in candle-light by a lens placed at a greater distance from the diffracting edge than its principal focus, the middle of the system of fringes corresponding to the diffracted shadow of a fibre is occupied with the *direct tint*, which we shall suppose to be *green*; and on each side of this *green* shadow, as we may call it, we observe very faintly the *complementary red* tinging what are called the two first exterior fringes. This tinge of red is strongest in the first fringe within the solid edge, or within the green shadow, while it is *yellowish* in the first fringe without the green shadow. These effects are inverted if we place the lens nearer to the edge than its principal focus.

The phenomena now described appear more distinct if we take an extremely narrow piece of nacrite, having its two edges nearly in contact, and transmitting only a narrow line of light. In this case the two red fringes within the solid edge unite their tints, and become a bright red; and in like manner if we place the lens nearer the solid edges than its principal focus, the two yellow fringes will unite their tints, and become a brighter yellow band. In this last case, when the two bounding edges are still nearer each other, the united fringes, in place of being yellow, will be *green*, or the same as the direct colour.

If we bring the edges of two pieces of nacrite of equal thickness very near each other, having, as formerly, *green* for the *direct*, and *red* for the *complementary* colour,

* Philosophical Transactions, 1836, p. 49.

the space between the edges, or between the green bands, will be faint *red* when the lens is nearer the edges than its principal focus, and *yellow* when it is further from them; but if the edges are brought still nearer, the faint red will become brighter, and the united green bands will take the place of the yellow one.

Let us now return to our plate of *nacrite* with a single edge, having *green* and *red* for the two tints; and let us always suppose that the lens is adjusted to observe the diffracted fringes, that is, that the lens is placed at a greater distance from the diffracting edge than its principal focus. We shall also suppose that the light of the sun passing through a narrow aperture parallel to the diffracting edge is substituted for the light of a candle. Under these circumstances the central part of the system of fringes seen by light incident perpendicularly, consists of *blue**, *green*, and *yellow* light, constituting, as it were, the shadow of the edge, the blue light being on the same side as the plate of nacrite, and the yellow rays encroaching upon the exterior faint red band already described, the other red band next the blue being more distinctly seen. If we now incline the incident ray to the plate of nacrite more than 90°, the faint red band next the yellow gradually becomes brighter, while the other bands become fainter; and at the boundary of light and darkness all the other bands disappear except this *red* one, which is the *complementary* colour to the *green*, (produced by the union of the *blue*, *green* and *yellow* bands,) and the colour which is seen upon the dark space next the edge of the flame, as described by Dr. Young. If we, on the other hand, incline the incident ray in an opposite direction, so that it forms with the plane of the plate a less angle than 90°, the *red* band next the blue will now become brighter; and at the boundary of light and darkness, when all the other bands have disappeared, the *red* band will afford the complementary colour to the *green*.

As the edge of the plate of nacrite is rough and unpolished, and accurately perpendicular to the parallel faces, there are no reflected nor refracted pencils, whose combinations with one another, or with the direct rays, can be employed to account for the complementary colours. The phenomena of mixed plates, indeed, are cases of diffraction when the light is obstructed by the edge of very thin transparent plates placed in a medium of different refractive power. If the plate were opake the fringes would be exactly those which have been so often described, and explained by the principle of interference. But owing to the *transparency* of the plate, fringes are produced within its shadow; and owing to the *thinness* of the plate the light transmitted through it and retarded, interferes with the partial waves which pass through the plate and with those which pass beyond the diffracting edge with undiminished velocity, and modifies the usual system of fringes in the manner which we have described.

As the plate of nacrite diminishes in thickness, or as the fluid in which it is immersed approaches to it in refractive density, the central coloured bands, whose union constitutes the *direct* tint, will diminish in number, and descending gradually in the scale will finally disappear when the retardation produced by the plate does not per-

* Owing to the small quantity of blue rays in candle-light the blue almost disappears in it.

ceptibly alter the phase of the ray. When the plate, on the other hand, increases in thickness, or the fluid diminishes in refractive power, the central bands will become closer and more numerous, and will finally resemble the fringes within the shadow of the ordinary system.

When the plate of nacrite is thicker at one place than another by the partial removal of a parallel film, the edge where the increase of thickness takes place produces exactly the same phenomena as the edge of the film that is removed, or of the film that is elevated above the general surface, and hence we are led to look for the phenomena of mixed plates in minerals, such as *sulphate of lime* and *mica*, where a plate of two different thicknesses can be easily obtained. I have accordingly discovered the phenomena of mixed plates distinctly exhibited in sulphate of lime and mica.

A more splendid exhibition of these colours is seen when a stratum of cavities of extreme thinness occurs in sulphate of lime. I have observed such strata repeatedly in the gypsum from Mont-martre; but they are most beautiful when the stratum has a circular form. In this case the cavities are exceedingly thin at the circumference of the circle, and gradually increase in depth towards the centre, so that we have a series of edges increasing in thickness towards a centre; the very reverse of a mixed plate, such as a film of albumen pressed between two convex surfaces. The system of rings is therefore also reversed, the highest order of colours being in the centre, while the lowest are at the circumference of the circular stratum. In many strata of cavities, such as the one which I have engraven in my paper on the new fluids in minerals*, the cavities are too deep to give the colours of mixed plates.

Another example of the colours of mixed plates in natural bodies occurs in specimens of mica, through which titanium is disseminated in beautiful flat dendritic crystals of various degrees of opacity and transparency. In these specimens the titanium is often disseminated in grains, forming an irregular surface. The edges of these grains, by retarding the light which they transmit, produce the direct and complementary colours of mixed plates in the most perfect manner, the tints passing through two orders of colours, as the grains of titanium increase in size towards the interior of the irregular patch. I have observed another example of these colours in the deep cavities of topaz, from which the fluids have either escaped, leaving one or both of the surfaces covered with minute particles of transparent matter, or in which the fluids have suffered induration.

Allerly by Melrose,
October 18*th*, 1837.

* Edinburgh Transactions, vol. x. Plate II. fig. 33.

VI. *Supplementary Note to Experimental Researches in Electricity.—Eleventh Series.*

By Michael Faraday, *Esq. D.C.L. F.R.S., Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c. &c.*

Received March 29, 1838.

1307. I HAVE recently put into an experimental form that general statement of the question of *specific inductive capacity* which is given at No. 1252 of Series XI., and the result is such as to lead me to hope the Council will authorise its addition to the paper in the form of a supplementary note. Three circular brass plates, about five inches in diameter, were mounted side by side upon insulating pillars; the middle one, A, was a fixture, but the outer plates B and C were moveable on slides, so that all three could be brought with their sides almost into contact, or separated to any required distance. Two gold leaves were suspended in a glass jar from insulated wires; one of the outer plates B was connected with one of the gold leaves, and the other outer plate with the other leaf. The outer plates B and C were adjusted at the distance of an inch and a quarter from the middle plate A, and the gold leaves were fixed at two inches apart; A was then slightly charged with electricity, and the plates B and C, with their gold leaves, thrown out of insulation *at the same time*, and then left insulated. In this state of things A was charged positive inductrically, and B with C negative inducteously; the same dielectric, air, being in the two intervals, and the gold leaves hanging, of course, parallel to each other in a relatively unelectrified state.

1308. A plate of shell-lac three quarters of an inch in thickness, and four inches square, suspended by clean white silk thread, was very carefully deprived of all charge (1203.), so that it produced no effect on the gold leaves if A were uncharged, and then introduced between plates A and B; the electric relation of the three plates was immediately altered, and the gold leaves attracted each other. On removing the shell-lac this attraction ceased; on introducing it between A and C it was renewed; on removing it the attraction again ceased; and the shell-lac when examined by a delicate Coulomb electrometer was still without charge.

1309. As A was positive, B and C were of course negative; but as the specific inductive capacity of shell-lac is about twice that of air (1270.), it was expected that when the lac was introduced between A and B, A would induce more towards B than towards C; that therefore B would become more negative than before towards A, and

consequently, because of its insulated condition, be positive externally, as at its back or at the gold leaves; whilst C would be less negative towards A, and therefore negative outwards or at the gold leaves. This was found to be the case; for on whichever side of A the shell-lac was introduced the external plate at that side was positive, and the external plate on the other side negative towards each other, and also to uninsulated external bodies.

1310. On employing a plate of sulphur instead of shell-lac, the same results were obtained; consistent with the conclusions drawn regarding the high specific inductive capacity of that body already given (1276.).

1311. These effects of specific inductive capacity can be exalted in various ways, and it is this capability which makes the great value of the apparatus. Thus I introduced the shell-lac between A and B, and then for a moment connected B and C, uninsulated them, and finally left them in the insulated state; the gold leaves were of course hanging parallel to each other. On removing the shell-lac the gold leaves attracted each other; on introducing the shell-lac between A and C this attraction was *increased*, (as had been anticipated from theory,) and the leaves came together, though not more than four inches long, and hanging three inches apart.

1312. By simply bringing the gold leaves nearer to each other I was able to show the difference of specific inductive capacity when only thin plates of shell-lac were used, the rest of the dielectric space being filled with air. By bringing B and C nearer to A another great increase of sensibility was made. By enlarging the size of the plates still further power was gained. By diminishing the extent of the wires, &c. connected with the gold leaves, another improvement resulted. So that in fact the gold leaves became, in this manner, as delicate a test of *specific inductive action* as they are, in Bennet's and Singer's electrometers, of ordinary electrical charge.

1313. It is evident that by making the three plates the sides of cells, with proper precautions as regards insulation, &c., this apparatus may be used in the examination of gases, with far more effect than the former apparatus (1187. 1290.), and may, perhaps, bring out differences which have as yet escaped me (1292. 1293.).

1314. It is also evident that two metal plates are quite sufficient to form the instrument; the state of the single inducteous plate when the dielectric is changed, being examined either by bringing a body excited in a known manner towards its gold leaves, or, what I think will be better, employing a carrier ball in place of the leaf, and examining that ball by the Coulomb electrometer (1180.). The inductive and inducteous surfaces may even be balls; the latter being itself the carrier ball of the Coulomb's electrometer (1181. 1229.).

1315. To increase the effect, a small condenser may be used with great advantage. Thus if, when two inducteous plates are used, a little condenser were put in the place of the gold leaves, I have no doubt the three principal plates might be reduced to an inch or even half an inch in diameter. Even the gold leaves act to each other for

the time as the plates of a condenser. If only two plates were used, by the proper application of the condenser the same reduction might take place. This expectation is fully justified by an effect already observed and described (1229.).

1316. In that case the application of the instrument to very extensive research is evident. Comparatively small masses of dielectrics could be examined, as diamonds and crystals. An expectation, that the specific inductive capacity of crystals will vary in different directions, according as the lines of inductive force (1304.) are parallel to, or in other positions in relation to the axes of the crystals, can be tested: I purpose that these and many other thoughts which arise respecting specific inductive action and the polarity of the particles of dielectric matter, shall be put to the proof as soon as I can find time.

1317. Hoping that this apparatus will form an instrument of considerable use, I beg to propose for it (at the suggestion of a friend) the name of *Differential Inductometer*.

Royal Institution,
March 29*th*, 1838.

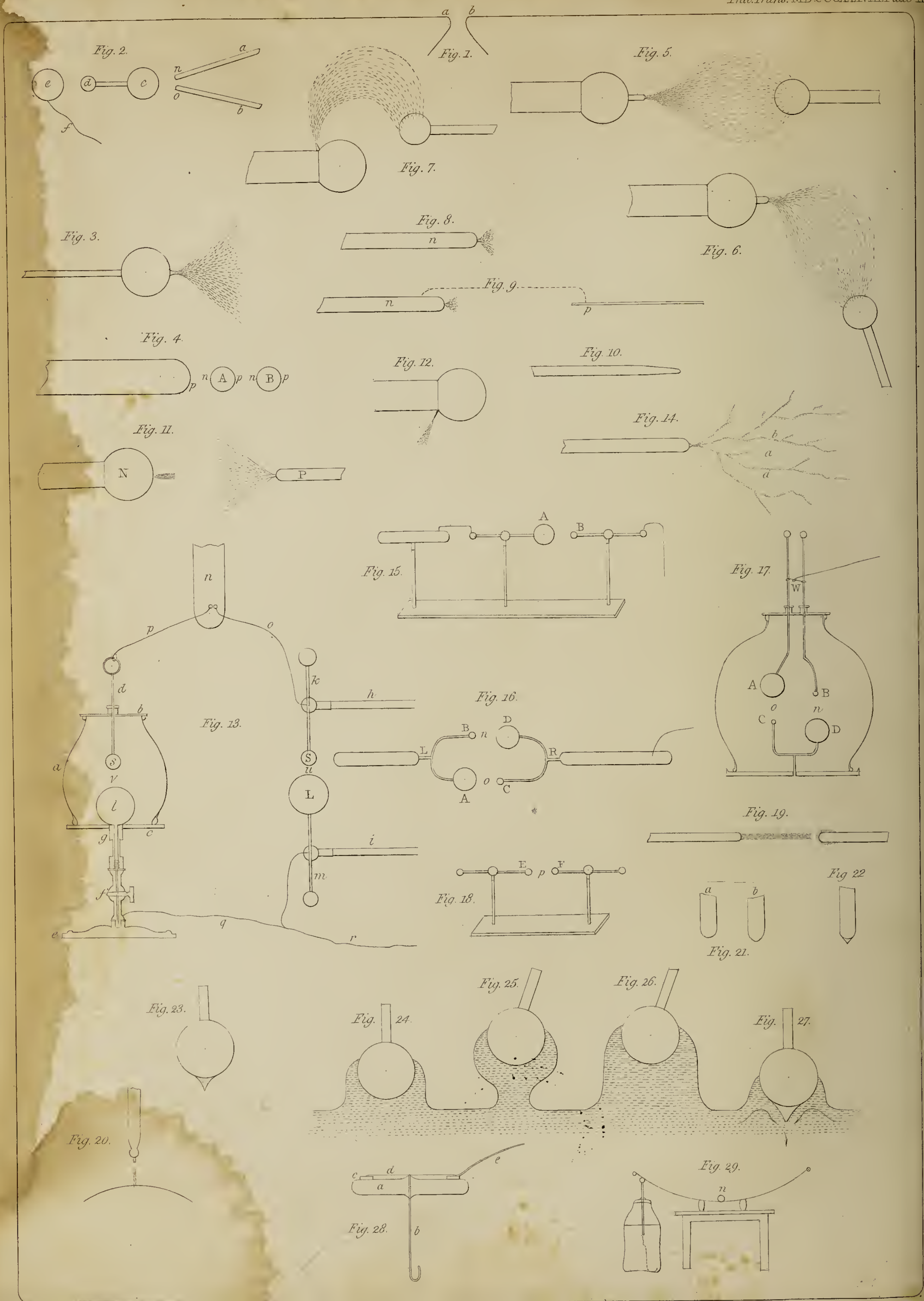
Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
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Fig. 29.

VII. *Experimental Researches in Electricity.—Twelfth Series.* By MICHAEL FARADAY, *Esq., D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c. &c.*

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§. 18. *On Induction* (*continued*). ¶ vii. *Conduction, or conductive discharge.* ¶ viii. *Electrolytic discharge.* ¶ ix. *Disruptive discharge—Insulation—Spark—Brush—Difference of discharge at the positive and negative surfaces of conductors.*

1318. I PROCEED now, according to my promise, to examine, by the great facts of electrical science, that theory of induction which I have ventured to put forth (1165. 1295. &c.). The principle of induction is so universal that it pervades all electrical phenomena; but the general case which I purpose at present to go into consists of insulation traced into and terminating with discharge, with the accompanying effects. This case includes the various *modes* of discharge, and also the condition and characters of a current; the elements of magnetic action being amongst the latter. I shall necessarily have occasion to speak theoretically, and even hypothetically; and though these papers profess to be experimental researches, I hope that, considering the facts and investigations contained in the last series in support of the particular view advanced, I shall not be considered as taking too much liberty on the present occasion, or as departing too far from the character which they ought to have, especially as I shall use every opportunity which presents itself of returning to that strong test of truth, experiment.

1319. Induction has as yet been considered in these papers only in cases of insulation;—opposed to insulation is *discharge*. The action or effect which may be expressed by the general term *discharge*, may take place, as far as we are aware at present, in several modes. Thus, that which is called simply *conduction* involves no chemical action, and apparently no displacement of the particles concerned. A second mode may be called *electrolytic discharge*; in it chemical action does occur, and particles must, to a certain degree, be displaced. A third mode, namely, that by sparks or brushes, may, because of its violent displacement of the particles of the *dielectric* in its course, be called the *disruptive discharge*; and a fourth may, perhaps, be conveniently distinguished for a time by the words *convection*, or *carrying discharge*, being that in which discharge is effected either by the carrying power of solid par-

ticles, or those of gases and liquids. Hereafter, perhaps, all these modes may appear as the result of one common principle, but at present they require to be considered apart; and I will now speak of the *first* mode, for amongst all the forms of discharge that which we express by the term conduction appears the most simple and the most directly in contrast with insulation.

¶ vii. *Conduction, or conductive discharge.*

1320. Though assumed to be essentially different, yet neither CAVENDISH nor POISSON attempt to explain by, or even state in, their theories, what the essential difference between insulation and conduction is. Nor have I anything, perhaps, to offer in this respect, *except* that, according to my view of induction, both it and conduction depend upon the same molecular action of the dielectrics concerned; are only extreme degrees of *one common condition* or effect; and in any sufficient mathematical theory of electricity must be taken as cases of the same kind. Hence the importance of the endeavour to show the connection between them under my theory of the electrical relations of contiguous particles.

1321. Though the action of the insulating dielectric in the charged Leyden jar, and that of the wire in discharging it, may seem very different, they may be associated by numerous intermediate links, which carry us on from one to the other, leaving, I think, no necessary connection unsupplied. We may observe some of these in succession for information respecting the whole case.

1322. Spermaceti has been examined and found to be a dielectric, through which induction can take place (1240. 1246.), its specific inductive capacity being about or above 1·8 (1279.), and the inductive action has been considered in it, as in all other substances, an action of contiguous particles.

1323. But spermaceti is also a *conductor*, though in so low a degree that we can trace the process of conduction, as it were, step by step through the mass (1247.); and even when the electric force has travelled through it to a certain distance, we can, by removing the coercitive (which is at the same time the inductive) force, cause it to return upon its path and reappear in its first place (1245. 1246.). Here induction appears to be a necessary preliminary to conduction. It of itself brings the contiguous particles of the dielectric into a certain condition, which, if retained by them, constitutes *insulation*, but if lowered by the communication of power from one particle to another, constitutes *conduction*.

1324. If *glass* or *shell-lac* be the substances under consideration, the same capabilities of suffering either induction or conduction through them appear (1233. 1239. 1247.), but not in the same degree. The conduction almost disappears (1239. 1242.); the induction therefore is sustained, i. e. the polarized state into which the inductive force has brought the contiguous particles is retained, there being little discharge action between them, and therefore the *insulation* continues. But, what discharge there is, appears to be consequent upon that condition of the particles into

which the induction throws them; and thus it is that ordinary insulation and conduction are closely associated together, or rather are extreme cases of one common condition.

1325. In ice or water we have a better conductor than spermaceti, and the phenomena of induction and insulation therefore quickly disappear, because conduction quickly follows upon the assumption of the inductive state. But let a plate of cold ice have metallic coatings on its sides, and connect one of these with a good electrical machine in work, and the other with the ground, and it then becomes easy to observe the phenomena of induction through the ice, by the electrical tension which can be obtained and continued on both the coatings (419. 426.). For although that portion of power which at one moment gave the inductive condition to the particles is at the next lowered by the consequent discharge due to the conductive act, it is succeeded by another portion of force from the machine to restore the inductive state. If the ice be converted into water, the same succession of actions can be just as easily proved, provided the water be distilled, and (if the machine be not powerful enough) a voltaic battery be employed.

1326. All these considerations impress my mind strongly with the conviction, that insulation and ordinary conduction cannot be properly separated when we are examining into their nature; that is, into the general law or laws under which their phenomena are produced. They appear to me to consist in an action of contiguous particles dependent on the forces developed in electrical excitement; these forces bring the particles into a state of tension or polarity, which constitutes both *induction* and *insulation*; and being in this state, the continuous particles have a power or capability of communicating their forces one to the other, by which they are lowered, and discharge occurs. Every body appears to discharge (444); but the possession of this capability in a *greater or smaller degree* in different bodies, makes them better or worse conductors, worse or better insulators; and both *induction* and *conduction* appear to be the same in their principle and action (1320.), except that in the latter an effect common to both is raised to the highest degree, whereas in the former it occurs in the best cases, in only an almost insensible quantity.

1327. That in our attempts to penetrate into the nature of electrical action, and to deduce laws more general than those we are at present acquainted with, we should endeavour to bring apparently opposite effects to stand side by side in harmonious arrangement, is an opinion of long standing, and sanctioned by the ablest philosophers. I hope, therefore, I may be excused the attempt to look at the highest cases of conduction as analogous to, or even the same in kind with, those of induction and insulation.

1328. If we consider the slight penetration of sulphur (1241. 1242.) or shell-lac (1234.) by electricity, or the feebler insulation sustained by spermaceti (1279. 1240.), as essential consequences and indications of their *conducting* power, then may we look on the resistance of metallic wires to the passage of electricity through them as

insulating power. Of the numerous well known cases fitted to show this resistance in what are called the perfect conductors, the experiments of Professor WHEATSTONE best serve my present purpose, since they were carried to such an extent as to show that *time* entered as an element into the conditions of conduction* even in metals. When discharge was made through a copper wire 2640 feet in length, and $\frac{1}{15}$th of an inch in diameter, so that the luminous sparks at each end of the wire, and at the middle, could be observed in the same place, the latter was found to be sensibly behind the two former in time, they being by the conditions of the experiment, simultaneous. Hence a proof of retardation; and what reason can be given why this retardation should not be of the same kind as that in spermaceti, or in lac, or sulphur? But as, in them, retardation is insulation, and insulation is induction, why should we refuse the same relation to the same exhibitions of force in the metals?

1329. We learn from the experiment, that if *time* be allowed the retardation is gradually overcome; and the same thing obtains for the spermaceti, the lac, and glass; give but time in proportion to the retardation, and the latter is at last vanquished. But if that be the case, and all the results are alike in kind, the only difference being in the length of time, why should we refuse to metals the previous inductive action, which is admitted to occur in the other bodies? The diminution of *time* is no negation of the action; nor is the lower degree of tension requisite to cause the forces to traverse the metal, as compared to that necessary in the cases of water, spermaceti, or lac. These differences would only point to the conclusion, that in metals the particles under induction can transfer their forces when at a lower degree of tension or polarity, and with greater facility than in the instances of the other bodies.

1330. Let us look at Mr. WHEATSTONE's beautiful experiment in another point of view. If, leaving the arrangement at the middle and two ends of the long copper wire unaltered, we remove the two intervening portions and replace them by wires of iron or platina, we shall have a much greater retardation of the middle spark than before. If, removing the iron, we were to substitute for it only five or six feet of water in a cylinder of the same diameter as the metal, we should have still greater retardation. If from water we passed to spermaceti, either directly or by gradual steps through other bodies, (even though we might vastly enlarge the bulk, for the purpose of evading the occurrence of a spark elsewhere (1331.) than at the three proper intervals,) we should have still greater retardation, until at last we might arrive, by degrees so small as to be inseparable from each other, at actual and permanent insulation. What, then, is to separate the principle of these two extremes, perfect conduction and perfect insulation, from each other; since the moment we leave in the smallest degree perfection at either extremity, we involve the element of perfection at the opposite end? Especially too, as we have not in nature the case of perfection either at one extremity or the other, either of insulation or conduction.

* Philosophical Transactions, 1834, p. 583.

1331. Again, to return to this beautiful experiment in the various forms which may be given to it: the forces are not all in the wire (after they have left the Leyden jar) during the whole time (1328.) occupied by the discharge; they are disposed in part through the surrounding dielectric under the well-known form of induction; and if that dielectric be air, induction takes place from the wire through the air to surrounding conductors, until the ends of the wire are electrically related through its length and discharge has occurred, i. e. for the *time* during which the middle spark is retarded beyond the others. This is well shown by the old experiment, in which a long wire is so bent that two parts (Plate III. fig. 1. *a. b.*) near its extremities shall approach within a short distance, as a quarter of an inch, of each other in the air. If the discharge of a Leyden jar, charged to a sufficient degree, be sent through such a wire, by far the largest portion of the electricity will pass as a spark across the air at the interval, and not by the metal. Does not the middle part of the wire, therefore, act here as an insulating medium, though it be of metal? and is not the spark through the air an indication of the tension (simultaneous with *induction*) of the electricity in the ends of this single wire? Why should not the wire and the air both be regarded as dielectrics; and the action at its commencement, and whilst there is tension, as an inductive action? If it acts through the contorted lines of the wire, so it also does in curved lines through air (1219. 1224.), and other insulating dielectrics (1228.); and we can apparently go so far in the analogy, whilst limiting the case to the inductive action only, as to show that amongst insulating dielectrics some lead away the lines of force from others (1229.), as the wire will do from worse conductors, though in it the principal effect is no doubt due to the ready discharge between the particles whilst in a low state of tension. The retardation is for the time insulation; and it seems to me we may just as fairly compare the air at the interval *a, b,* (fig. 1.) and the wire in the circuit, as two bodies of the same kind and acting upon the same principles, as far as the first inductive phenomena are concerned, notwithstanding the different forms of discharge which ultimately follow*, as we may compare, according to Coulomb's investigations†, *different lengths* of different insulating bodies required to produce the same amount of insulating effect.

1332. This comparison is still more striking when we take into consideration the experiment of Mr. Harris, in which he stretched a fine wire across a glass globe, the air within being rarefied‡. On sending a charge through the joint arrangement of metal and rare air, as much, if not more, electricity passed by the latter as by the former. In the air, rarefied as it was, there can be no doubt the discharge was preceded by induction (1284.); and to my mind all the circumstances indicate that the same was the case with the metal; that, in fact, both substances are dielectrics, ex-

* These will be examined hereafter (1348, &c.).

† Mémoires de l'Académie, 1785, p. 612. or Ency. Britann. First Supp. vol. i. p. 611.

‡ Philosophical Transactions, 1834, p. 242.

hibiting the same effects in consequence of the action of the same causes, the only variation being one of degree in the different substances employed.

1333. Judging on these principles, velocity of discharge through the *same wire* may be varied greatly by attending to the circumstances which cause variations of discharge through spermaceti or sulphur. Thus, for instance, it must vary with the tension or intensity of the first urging force (1234. 1240.), which tension is charge and induction. So if the two ends of the wire, in Professor WHEATSTONE's experiment, were immediately connected with two large insulated metallic surfaces exposed to the air, so that the primary act of induction, after making the contact for discharge, might be in part removed from the internal portion of the wire at the first instant, and disposed for the moment on its surface jointly with the air and surrounding conductors, then I venture to anticipate that the middle spark would be more retarded than before; and if these two plates were the inner and outer coating of a large jar or a Leyden battery, then the retardation of that spark would be still greater.

1334. CAVENDISH was perhaps the first to show distinctly that discharge was not always by one channel*, but, if several are present, by many at once. We may make these different channels of different bodies, and by proportioning their thicknesses and lengths, may include such substances as air, lac, spermaceti, water, protoxide of iron, iron and silver, and by *one* discharge make each convey its proportion of the electric force. Perhaps the air ought to be excepted, as its discharge by conduction is questionable at present; but the others may all be limited in their mode of discharge to pure conduction. Yet several of them suffer previous induction, precisely like the induction through the air, it being a necessary preliminary to their discharging action. How can we therefore separate any one of these bodies from the others, as to the *principles and mode* of insulating and conducting, except by mere degree? All seem to me to be dielectrics acting alike, and under the same common laws.

1335. I might draw another argument in favour of the general sameness, in nature and action, of good and bad conductors (and all the bodies I refer to are conductors more or less), from the perfect equipoise in action of very different bodies when opposed to each other in magneto-electric inductive action, as formerly described (213.), but am anxious to be as brief as is consistent with the clear examination of the probable truth of my views.

1336. With regard to the possession by the gases of any conducting power of the simple kind now under consideration, the question is a very difficult one to determine at present. Experiments seem to indicate that they do insulate certain low degrees of tension perfectly, and that the effects which may have appeared to be occasioned by *conduction* have been the result of the carrying power of the charged particles,

* Philosophical Transactions, 1776, p. 197.

either of the air or of dust, in it. It is equally certain, however, that with higher degrees of tension or charge they discharge to one another, and that is conduction. If they possess the power of insulating a certain low degree of tension continuously and perfectly, such a result may be due to their peculiar physical state, and the condition of separation under which their particles are placed. But in that, or in any case, we must not forget the fine experiments of Cagniard de la Tour*, in which he has shown that liquids and their vapours can be made to pass gradually into each other, to the entire removal of any marked distinction of the two states. Thus, hot dry steam and cold water pass by insensible gradations into each other; yet the one is amongst the gases as an insulator, and the other a comparatively good conductor. As to conducting power, therefore, the transition from metals even up to gases is gradual; substances make but one series in this respect, and the various cases must come under one condition and law (444.). The specific differences of bodies as to conducting power only serves to strengthen the general argument, that conduction, like insulation, is a result of induction, and is an action of contiguous particles.

1337. I might go on now to consider induction and its concomitant, *conduction*, through mixed dielectrics, as, for instance, when a charged body, instead of acting across air to a distant uninsulated conductor, acts jointly through it and an interposed insulated conductor. In such a case, the air and the conducting body are the mixed dielectrics; and the latter assumes a polarized condition as a mass, like that which my theory assumes *each particle* of the air to possess at the same time. But I fear to be tedious in the present condition of the subject, and hasten to the consideration of other matter.

1338. To sum up, in some degree, what has been said, I look upon the first effect of an excited body upon neighbouring matters to be the production of a polarized state of their particles, which constitutes *induction*; and this arises from its action upon the particles in immediate contact with it, which again act upon those contiguous to them, and thus the forces are transferred to a distance. If the induction remain undiminished, then perfect insulation is the consequence; and the higher the polarized condition which the particles can acquire or maintain, the higher is the intensity which may be given to the acting forces. If, on the contrary, the contiguous particles, upon acquiring the polarized state, have the power to communicate their forces, then conduction occurs, and the tension is lowered, conduction being a distinct act of discharge between neighbouring particles. The lower the state of tension at which this discharge between the particles of a body takes place, the better conductor is that body. In this view, insulators may be said to be bodies whose particles can retain the polarized state; whilst conductors are those whose particles cannot be permanently polarized. If I be right in my view of induction, then I consider the reduction of these two effects (which have been so long held distinct) to an action of

* Annales de Chimie, xxi. pp. 127. 178. or Quarterly Journal of Science, xv. 145.

contiguous particles obedient to one common law, as a very important result; and, on the other hand, the identity of character which the two acquire when viewed by the theory (1326.), is additional presumptive proof in favour of the correctness of the latter.

1339. That heat has great influence over simple conduction is well known (445.), its effect being, in some cases, almost an entire change of the characters of the body (432. 1340.). Harris has, however, shown that it in no respect affects gaseous bodies, or at least air*; and Davy has taught us that, as a class, metals have their conducting power *diminished* by it†.

1340. I formerly described a substance, sulphuret of silver, whose conducting power was increased by heat (433. 437. 438.); and I have since then met with another as strongly affected in the same way: this is fluoride of lead. When a piece of that substance, which had been fused and cooled, was introduced into the circuit of a voltaic battery, it stopped the current. Being heated, it acquired conducting powers before it was visibly red hot in daylight; and even sparks could be taken against it whilst still solid. The current alone then raised its temperature (as in the case of sulphuret of silver) until it fused, after which it seemed to conduct as well as the metallic vessel containing it; for whether the wire used to complete the circuit touched the fused fluoride only, or was in contact with the platina on which it was supported, no sensible difference in the current was observed. During all the time there was scarcely a trace of decomposing action on the fluoride, and what did occur, seemed referable to the air and moisture of the atmosphere, and not to electrolytic action.

1341. I have now very little doubt that periodide of mercury (414. 448. 691.) is a case of the same kind, and also corrosive sublimate (692.). I am also inclined to think, since making the above experiments, that the anomalous action of the protoxide of antimony, formerly observed and described (693. 801.), may be referred in part to the same cause.

1342. I have no intention at present of going into the particular relation of heat and electricity, but we may hope hereafter to discover by experiment the law which probably holds together all the above effects with those of the *evolution* and the *disappearance* of heat by the current, and the striking and beautiful results of thermo-electricity, in one common bond.

¶ viii. *Electrolytic discharge.*

1343. I have already expressed in a former paper (1164.) the view by which I hope to associate ordinary induction and electrolyzation. Under that view, the discharge of electric forces by electrolyzation is rather an effect superadded, in a certain class

* Philosophical Transactions, 1834, p. 230.

† Ibid. 1821, p. 431.

of bodies, to those already described as constituting induction and insulation, than one independent of and distinct from these phenomena.

1344. Electrolytes, as respects their insulating and conducting forces, belong to the general category of bodies (1320. 1334.); and if they are in the solid state (as nearly all can assume that state), they retain their place, presenting then no new phenomenon (426, &c.); or if one occur being in so small a proportion as to be almost unimportant. When liquefied, they also belong to the same list whilst the electric intensity is below a certain degree; but at a given intensity (910. 912. 1007.), fixed for each, and very low in all known cases, they play a new part, causing discharge in proportion (783.) to the development of certain chemical effects of combination and decomposition; and at this point, move out from the general class of insulators and conductors, to form a distinct one by themselves. The former phenomena have been considered (1320. 1338.); it is the latter which have now to be revised, and used as a test of the proposed theory of induction.

1345. The theory assumes, that the particles of the dielectric (now an electrolyte) are in the first instance brought, by ordinary inductive action, into a polarized state, and raised to a certain degree of tension or intensity before discharge commences; the inductive state being, in fact, a *necessary preliminary* to discharge. By taking advantage of those circumstances which bear upon the point, it is not difficult to increase the tension indicative of this state of induction, and so make the state itself more evident. Thus, if distilled water be employed, and a long narrow portion of it placed between the electrodes of a powerful voltaic battery, we have at once indications of the intensity which can be sustained at these electrodes by the inductive action through the water as a dielectric, for sparks may be obtained, gold leaves diverged, and Leyden bottles charged at their wires. The water is in the condition of the spermaceti (1322. 1323.), a bad conductor and a bad insulator; but what it does insulate is by virtue of inductive action, and that induction is the preparation for and precursor of discharge (1338.).

1346. The induction and tension which appear at the limits of the portion of water in the direction of the current, are only the sums of the induction and tension of the contiguous particles between those limits; and the limitation of the inductive tension, to a certain degree shows (time entering in each case as an important element of the result), that when the particles have acquired a certain relative state, *discharge*, or a transfer of forces equivalent to ordinary conduction, takes place.

1347. In the inductive condition assumed by water before discharge comes on, the particles polarized are the particles of the *water*, that being the dielectric used; but the discharge between particle and particle is not, as before, a mere interchange of their powers or forces at the polar parts, but an actual separation of them into their two elementary particles, the oxygen travelling in one direction, and carrying with it its amount of the force it had acquired during the polarization, and the hydrogen doing the same thing in the other direction, until they each meet the next approaching

particle, which is in the same electrical state with that they have left, and by association of their forces with it, produce what constitutes discharge. This part of the action may be regarded as a carrying one (1319.), performed by the constituent particles of the dielectric. The latter is always a compound body (664. 823.); and by those who have considered the subject and are acquainted with the philosophical view of transfer which was first put forth by Grotthuss*, its particles may easily be compared to a series of metallic conductors under inductive action, which, whilst in that state, are divisible into these elementary moveable halves.

1348. Electrolytic discharge depends, of necessity, upon the non-conduction of the dielectric as a whole, and there are two steps or acts in the process: first a polarization of the molecules of the substance, and then a lowering of the forces by the separation, advance in opposite directions, and recombination of the elements of the molecules, they being, as it were, the halves of the originally polarized conductors or particles.

1349. These views of the decomposition of electrolytes and the consequent effect of discharge, which, as to the particular case, are the same with those of Grotthuss (481.) and Davy (482.), though they differ from those of Biot (487.), De la Rive (490.), and others, seem to me to be fully in accordance not merely with the theory I have given of induction generally (1165.), but with all the known *facts* of common induction, conduction, and electrolytic discharge; and in that respect help to confirm, in my mind, the truth of the theory set forth. The new mode of discharge which electrolyzation presents must surely be an evidence of the *action of contiguous particles*; and as this appears to depend directly upon a previous inductive state, which is the same with common induction, it greatly strengthens the argument which refers induction in all cases to an action of contiguous particles also (1295, &c.).

1350. As an illustration of the condition of the polarized particles in a dielectric under induction, I may describe an experiment. Put into a glass vessel some clear rectified oil of turpentine, and introduce two wires passing through glass tubes where they are at the surface of the fluid, and terminating either in balls or points. Cut some very clean dry white silk into small particles, and put these also into the liquid; then electrify one of the wires by an ordinary machine and discharge by the other. The silk will immediately gather from all parts of the liquid, and form a band of particles reaching from wire to wire, and if touched by a glass rod will show considerable tenacity; yet the moment the supply of electricity ceases, the band will fall away and disappear by the dispersion of its parts. The *conduction* by the silk is in this case very small; and after the best examination I could give to the effects, the impression on my mind is, that the adhesion of the whole is due to the polarity which each filament acquires, exactly as the particles of iron between the poles of a horse-shoe magnet are held together in one mass by a similar disposition of forces. The particles of silk therefore represent to me the condition of the molecules of the dielectric itself, which I assume to be polar, just as that of the silk is. In all cases of conductive dis-

* Annales de Chimie, lviii. 60. and lxiii. 20.

charge the contiguous polarized particles of the body are able to effect a neutralization of their forces with greater or less facility, as the silk does also in a very slight degree. Further we are not able to carry the parallel, except in imagination; but if we could divide each particle of silk into two halves, and let each half travel until it met and united with the next half in an opposite state, it would then exert its carrying power (1347.), and so far represent electrolytic discharge.

1351. Admitting that electrolytic discharge is a consequence of previous induction, then how evidently do its numerous cases point to induction in curved lines (1216.), and to the divergence or lateral action of the lines of inductive force (1231.), and so strengthen that part of the general argument in the former paper! If two balls of platina, forming the electrodes of a voltaic battery, are put into a large vessel of dilute sulphuric acid, the whole of the surfaces are covered with the respective gases in beautifully regulated proportions, and the mind has no difficulty in conceiving the direction of the curved lines of discharge, and even the intensity of force of the different lines, by the quantity of gas evolved upon the different parts of the surface. Hence the general effects of diffusion; the appearance of the anions or cathions round the edges and on the further side of the electrodes when in the form of plates; the manner in which the current or discharge will follow all the forms of the electrolyte, however contorted. Hence the effects which Nobili has so well examined and described* in his papers on the distribution of currents in conducting masses. All these effects indicate the direction of the currents or discharges which occur in and through the dielectrics, and these are in every case *preceded* by equivalent inductive actions of the contiguous particles.

1352. Hence also the advantage, when the exciting forces are weak or require assistance, of enlarging the mass of the electrolyte; of increasing the size of the electrodes; of making the coppers surround the zincs:—all is in harmony with the view of induction which I am endeavouring to examine; I do not perceive as yet one fact against it.

1353. There are many points of *electrolytic discharge* which ultimately will require to be very closely considered, though I can but slightly touch upon them. It is not that, as far as I have investigated them, they present any contradiction to the view taken (for I have carefully, though unsuccessfully, sought for such cases), but simply want of time as yet to pursue the inquiry, which prevents me from entering upon them here.

1354. One point is, that different electrolytes or dielectrics require different initial intensities for their decomposition (912.). This may depend upon the degree of polarization which the particles require before electrolytic discharge commences. It is in direct relation to the chemical affinity of the substances concerned; and will probably be found to have a relation or analogy to the specific inductive capacity of different bodies (1252. 1296.). It thus promises to assist in causing the great truths

* Bibliotheque Universelle, 1835, lix. 263. 416.

of those extensive sciences, which are occupied in considering the forces of the particles of matter, to fall into much closer order and arrangement than they have heretofore presented.

1355. Another point is, the facilitation of electrolytic conducting power or discharge by the addition of substances to the dielectric employed. This effect is strikingly shown where water is the body whose qualities are improved, but, as yet, no general law governing all the phenomena has been detected. Thus some acids, as the sulphuric, phosphoric, oxalic, and nitric, increase the power of water enormously; whilst others, as the tartaric and citric acids, give but little power; and others, again, as the acetic and boracic acids, do not produce a change sensible to the voltameter (739.). Ammonia produces no effect, but its carbonate does. The caustic alkalies and their carbonates produce a fair effect. Sulphate of soda, nitre (753.), and many soluble salts produce much effect. Percyanide of mercury and corrosive sublimate produce no effect; nor does iodine, gum, or sugar, the test being a voltameter. In many cases the added substance is acted on either directly or indirectly, and then the phenomena are more complicated; such substances are muriatic acid (758.), the soluble protochlorides, (766.), and iodides (769.), nitric acid (752.), &c. In other cases the substance added is not, when alone, subject to or a conductor of the powers of the voltaic battery, and yet both gives and receives power when associated with water. M. DE LA RIVE has pointed this result out in sulphurous acid*, iodine and bromine†; the chloride of arsenic produces the same effect. A far more striking case, however, is presented by that very influential body sulphuric acid (681.); and probably phosphoric acid also is in the same peculiar relation.

1356. It would seem in the cases of those bodies which suffer no change themselves, as sulphuric acid (and perhaps in all), that they affect water in its conducting power only as an electrolyte; for whether little or much improved, the decomposition is proportionate to the quantity of electricity passing (727. 730.), and the transfer is therefore due to electrolytic discharge. This is in accordance with the fact already stated as regards water (984.), that the conducting power is not improved for electricity of force below the electrolytic intensity of the substance acting as the dielectric; but both facts (and some others) are against the opinion which I formerly gave, that the power of salts, &c. might depend upon their assumption of the liquid state by solution in the water employed (410.). It occurs to me that the effect may perhaps be related to, and have its explanation in differences of specific inductive capacities.

1357. I have described in the last paper, cases, where shell-lac was rendered a conductor by absorption of ammonia (1294.). The same effect happens with muriatic acid; yet both these substances, when gaseous, are non-conductors; and the

* Quarterly Journal, xxvii. 407. or Bibliotheque Universelle, xl. 205. KEMP says sulphurous acid is a very good conductor, Quarterly Journal, 1831, p. 613.

† Quarterly Journal, xxiv. 465. or Annales de Chimie, xxxv. 161.

ammonia, also when in strong solution (748.). Mr. Harris has mentioned instances* in which the conducting power of metals is seriously altered by a very little alloy. These may have no relation to the former cases, but nevertheless should not be overlooked in the general investigation which the whole question requires.

1358. Nothing is perhaps more striking in that class of dielectrics which we call electrolytes, than the extraordinary and almost complete suspension of their peculiar mode of effecting discharge when they are rendered *solid* (380, &c.), even though the intensity of the induction acting through them may be increased a hundred fold or more (419.). It not only establishes a very general relation between the physical properties of these bodies and electricity acting by induction through them, but draws both their physical and chemical relations so near together, as to make us hope we shall shortly arrive at the full comprehension of the influence they mutually possess over each other.

¶ ix. *Disruptive discharge and insulation.*

1359. The next form of discharge has been distinguished by the adjective *disruptive* (1319.), as it in every case displaces more or less the particles amongst and across which it suddenly breaks. I include under it, discharge in the form of sparks, brushes, and glow (1405.), but exclude the cases of currents of air, fluids, &c., which, though frequently accompanying the former, are essentially distinct in their nature.

1360. The conditions requisite for the production of an electric spark in its simplest form are well known. An insulating dielectric must be interposed between two conducting surfaces in opposite states of electricity, and then if the actions be continually increased in strength, or otherwise favoured, either by exalting the electric state of the two conductors, or bringing them nearer to each other, or diminishing the density of the dielectric, a *spark* at last appears, and the two forces are for the time annihilated, for *discharge* has occurred.

1361. The conductors (which may be considered as the termini of the inductive action) are in ordinary cases most generally metals, whilst the dielectrics usually employed are common air and glass. In my view of induction, however, every dielectric becomes of importance, for as the results are considered essentially dependent on these bodies, it was to be expected that differences of action never before suspected would be evident upon close examination, and so at once give fresh confirmation of the theory, and open new doors of discovery into the extensive and varied fields of our science. This hope was especially entertained with respect to the gases, because of their high degree of insulation, their uniformity in physical condition, and great difference in chemical properties.

1362. All the effects prior to the discharge are inductive; and the degree of tension which it is necessary to attain before the spark passes is therefore, in the examination I am now making of the new view of induction, a very important point. It is the

* Philosophical Transactions, 1827, p. 22.

limit of the influence which the dielectric exerts in resisting discharge; it is a measure, consequently, of the conservative power of the dielectric, which in its turn may be considered as becoming a measure, and therefore a representative of the intensity of the electric forces in activity.

1363. Many philosophers have examined the circumstances of this limiting action in air, but, as far as I know, none have come near Mr. Harris as to the accuracy with, and the extent to, which he has carried on his investigations*. Some of his results I must very briefly notice, premising that they are all obtained with the use of air as the *dielectric* between the conducting surfaces.

1364. First as to the *distance* between the two balls used, or in other words, the *thickness* of the dielectric across which the induction was sustained. The quantity of electricity, measured by a unit jar or otherwise on the same principle with the unit jar, in the charged or inductive ball, necessary to produce spark discharge, was found to vary exactly with the distance between the balls, or between the discharging points, and that under very varied and exact forms of experiment†.

1365. Then with respect to variation in the *pressure or density* of the air. The quantities of electricity required to produce discharge across a *constant* interval varied exactly with variations of the density; the quantity of electricity and density of the air being in the same simple ratio. Or, if the quantity was retained the same, whilst the interval and density of the air were varied, then these were found in the inverse simple ratio of each other, the same quantity passing across twice the distance with air rarefied to one half‡.

1366. It must be remembered that these effects take place without any variation of the inductive force by condensation or rarefaction of the air. That force remains the same in air§, and in all gases (1284. 1292.), whatever their rarefaction may be.

1367. Variation of the *temperature* of the air produced no variation of the quantity of electricity required to cause discharge across a given interval||.

Such are the general results, which I have occasion for at present, obtained by Mr. Harris, and they appear to me to be unexceptionable.

1368. In the theory of induction founded upon a molecular action of the dielectric, we have to look to the state of that body principally for the cause and determination of the above effects. Whilst the induction continues, it is assumed that the particles of the dielectric are in a certain polarized state, the tension of this state rising higher in each particle as the induction is raised to a higher degree, either by approximation of the inducing surfaces, variations of form, increase of the original force, or other means; until at last, the tension of the particles having reached the utmost degree which they can sustain without subversion of the whole arrangement, discharge immediately after takes place.

1369. The theory does not assume, however, that *all* the particles of the dielectric

* Philosophical Transactions, 1834, p. 225. † Ibid. ‡ Ibid. p. 229.
§ Ibid. pp. 237. 244. || Ibid. p. 230.

subject to the inductive action are affected to the same amount, or acquire the same tension. What has been called the lateral action of the lines of inductive force (1231. 1297.), and the diverging and occasionally curved form of these lines, is against such a notion. The idea is, that any section taken through the dielectric across the lines of inductive force, and including *all of them*, would be equal, in the sum of the forces, to the sum of the forces in any other section; and that, therefore, the whole amount of tension for each such section would be the same.

1370. Discharge probably occurs, not when all the particles have attained to a certain degree of tension, but when that particle which is most affected has been exalted to the subverting or turning point (1410.). For though *all* the particles in the line of induction resist charge, and are associated in their actions so as to give a sum of resisting force, yet when any one is brought up to the overturning point, *all* must give way in the case of a spark between ball and ball. The breaking down of that one must of necessity cause the whole barrier to be overturned, for it was at its utmost degree of resistance when it possessed the aiding power of that one particle, in addition to the power of the rest, and the power of that one is now lost. Hence *tension* or *intensity** may, according to the theory, be considered as represented by the particular condition of the particles, or the amount in them of forced variation from their normal state (1298. 1368.).

1371. The whole effect produced by a charged conductor on a distant conductor, insulated or not, is by my theory assumed to be due to an action propagated from particle to particle of the intervening and insulating dielectric, the particles being considered as thrown for the time into a forced condition, from which they endeavour to return to their normal or natural state. The theory, therefore, seems to supply an easy explanation of the influence of *distance* in affecting induction (1303. 1364.). As the distance is diminished induction increases; for there are then fewer particles in the line of inductive force to oppose their resistance to the assumption of the forced or polarized state, and *vice versâ*. Again, as the distance diminishes, discharge across happens with a lower charge of electricity; for if, as in Harris's experiments (1364.), the interval be diminished to one half, then half the electricity required to discharge across the first interval is sufficient to strike across the second; and it is evident, also, that at that time there are only half the number of interposed molecules uniting their forces to resist the discharge.

1372. The effect of enlarging the conducting surfaces which are opposed to each other in the act of induction, is, if the electricity be limited in its supply, to lower the intensity of action; and this follows as a very natural consequence from the increased area of the dielectric across which the induction is effected. For by diffusing the inductive action, which at first was exerted through one square inch of sectional area of the dielectric, over two or three square inches of such area, twice or three times the number of molecules of the dielectric are brought into the polarized con-

* See Harris on proposed particular meaning of these terms, Philosophical Transactions, 1834, p. 222.

dition, and employed in sustaining the inductive action, and consequently the tension belonging to the smaller number on which the limited force was originally accumulated, must fall in a proportionate degree.

1373. For the same reason diminishing these opposing surfaces must increase the intensity up to the condition even of their becoming points. But in this case, the tension of the particles of the dielectric next the points is higher than that of particles midway, because of the lateral action and consequent bulging, as it were, of the lines of inductive force at the middle distance (1369.).

1374. The more exalted effects of induction on a point *p*, or any small surface, as the rounded end of a rod, opposed to a large surface, as that of a ball or plate, than when it is opposed to another point or end at the same distance, falls into harmonious relation (1302.). For in the latter case, the small surface *p* is affected only by those particles which are brought into the inductive condition by the equally small surface of the opposed conductor, whereas when that is a ball or plate the lines of inductive force from the latter are concentrated, as it were, upon the end *p*. Now though the molecules of the dielectric against the large surface may have a much lower state of tension than those against the similar smaller surface, yet they are also far more numerous, and, as the lines of inductive force converge towards a point, are able to communicate to the particles contained in any cross section (1369.) nearer the small surface an amount of tension equal to their own, and consequently much higher for each individual particle; so that, at the surface of the smaller conductor, the tension of a particle rises much, and if that conductor were to terminate in a point, the tension would rise to an infinite degree, except that it is limited, as before (1368.), by discharge. The nature of the discharge from small surfaces and points under induction will be resumed hereafter (1425. &c.).

1375. *Rarefaction* of the air does not alter the *intensity* of inductive action (1284. 1287.); nor is there any reason, as far as I can perceive, why it should. If the quantity of electricity and the distance remain the same, and the air be rarefied one half, then, though one half of the particles of the dielectric are removed, the other half assume a double degree of tension in their polarity, and therefore the inductive forces are balanced, and the result remains unaltered as long as the induction and insulation are sustained. But the case of *discharge* is very different; for as there are only half the number of dielectric particles in the rarefied atmosphere, so these are brought up to the discharging intensity by half the former quantity of electricity; discharge, therefore, ensues, and such a consequence of the theory is in perfect accordance with Mr. Harris's results (1365.).

1376. The *increase* of electricity required to cause discharge over the same distance, when the pressure of the air or its density is increased, flows in a similar manner, and on the same principle, from the molecular theory.

1377. Here I think my view of induction has a decided advantage over others, especially over that which refers the retention of electricity on the surface of conductors

in air to the *pressure of the atmosphere*. The latter is the view which, being adopted by Poisson and Biot*, is also, I believe, that generally received; and it relates two such dissimilar things, as the ponderous air and the subtil and even hypothetical fluid or fluids of electricity, by gross mechanical relations; by the bonds of mere static pressure. My theory, on the contrary, sets out at once by connecting the electric forces with the particles of matter; it derives all its proofs, and even its origin in the first instance, from experiment; and then, without any further assumption, seems to offer at once a full explanation of these and many other singular, peculiar, and, I think, heretofore unrelated effects.

1378. An important assisting experimental argument may here be adduced, derived from the difference of specific inductive capacity of different dielectrics (1269. 1274. 1278.). Consider an insulated sphere electrified positively and placed in the centre of another and larger sphere uninsulated, a uniform dielectric, as air, intervening. The case is really that of my apparatus (1187.), and also, in effect, that of any ball electrified in a room and removed to some distance from irregularly formed conductors. Whilst things remain in this state the electricity is distributed (so to speak) uniformly over the surface of the electrified sphere. But introduce such a dielectric as sulphur or lac, into the space between the two conductors on one side only, or opposite one part of the inner sphere, and immediately the electricity on the latter is diffused unequally (1229. 1270. 1309.), although the form of the conducting surfaces, their distances, and the *pressure* of the atmosphere remain perfectly unchanged.

1379. Fusinieri took a different view from that of Poisson, Biot, and others, of the reason why rarefaction of air caused easy diffusion of electricity. He considered the effect as due to the removal of the *obstacle* which the air presented to the expansion of the substances from which the electricity passed†. But platina balls show the phenomena in vacuo as well as volatile metals and other substances; besides which, when the rarefaction is very considerable, the electricity passes with scarcely any resistance, and the production of no sensible heat; so that I think Fusinieri's view of the matter is likely to gain but few assents.

1380. I have no need to remark upon the discharging or collecting power of flame or hot air. I believe, with Harris, that the mere heat does nothing (1367.), the rarefaction only being influential. The effect of rarefaction has been already considered generally (1375.); and that caused by the heat of a burning light, with the pointed form of the wick, and the carrying power of the carbonaceous particles which for the time are associated with it, are fully sufficient to account for all the effects.

1381. We have now arrived at the important question, how will the inductive tension requisite for insulation and disruptive discharge be sustained in gases, which,

* Ency. Britann. Supplement, vol. iv. Article Electricity, pp. 76. 81. &c.

† Bib. Univ. 1831. xlviii. 375.

having the same physical state and also the *same pressure* and the *same temperature* as *air*, differ from it in specific gravity, in chemical qualities, and it may be in peculiar relations, which not being as yet recognised, are purely electrical (1361.)?

1382. Into this question I can enter now only as far as is essential for the present argument, namely, that insulation and inductive tension do not depend merely upon the charged conductors employed, but also, and essentially, upon the interposed dielectric, in consequence of the molecular action of its particles.

1383. A glass vessel *a* (fig. 13.)* was ground at the top and bottom so as to be closed by two ground brass plates, *b* and *c*; *b* carried a stuffing box, with a sliding rod *d* terminated by a brass ball *s* below, and a ring above. The lower plate was connected with a foot, stop-cock, and socket, *e, f* and *g*; and also with a brass ball *l*, which by means of a stem attached to it and entering the socket *g*, could be fixed at various heights. The metallic parts of this apparatus were not varnished, but the glass was well covered with a coat of shell-lac previously dissolved in alcohol. On exhausting the vessel at the air-pump it could be filled with any other gas than air, and, in such cases, the gas so passed in was dried whilst entering by fuzed chloride of calcium.

1384. The other part of the apparatus consisted of two insulating pillars, *h* and *i*, to which were fixed two brass balls, and through these passed two sliding rods, *k* and *m*, terminated at each end by brass balls; *n* is the end of an insulated conductor, which could be rendered either positive or negative from an electrical machine; *o* and *p* are wires connecting it with the two parts previously described, and *q* is a wire which, connecting the two opposite sides of the collateral arrangements, also communicates with a good discharging train *r* (292.).

1385. It is evident that the discharge from the machine electricity may pass either between *s* and *l*, or S and L. The regulation adopted in the first experiments was to keep *s* and *l* with their distance *unchanged*, but to introduce first one gas and then another into the vessel *a*, and then balance the discharge at the one place against that at the other; for by making the interval at *u* sufficiently small, all the discharge would pass there, or making it sufficiently large it would all occur at the interval *v* in the receiver. On principle it seemed evident, that in this way the varying interval *u* might be taken as a measure, or rather indication of the resistance to discharge through the gas at the constant interval *v*. The following are the constant dimensions.

Ball *s*	0·93 of an inch.
Ball S	0·96 of an inch.
Ball *l*	2·02 of an inch.
Ball L	1·95 of an inch.
Interval *v*	0·62 of an inch.

1386. On proceeding to experiment it was found that when air or any gas was in

* The drawing is to a scale of $\frac{1}{6}$.

the receiver *a*, the interval *u* was not a fixed one; it might be altered through a certain range of distance, and yet sparks pass either there or at *v* in the receiver. The extremes were therefore noted, i. e. the greatest distance short of that at which the discharge *always* took place at *v* in the gas, and the least distance short of that at which it *always* took place at *u* in the air. Thus, with air in the receiver, the extremes at *u* were 0·56 and 0·79 of an inch, the range of 0·23 being one at which sparks passed occasionally either at one interval or the other.

1387. The small balls *s* and S could be rendered either positive or negative from the machine, and as gases were expected and were found to differ from each other in relation to this change, the results obtained under these differences of charge were also noted.

1388. The following is a Table of results; the gas named is that in the vessel *a*. The smallest, greatest, and mean interval at *u* in air is expressed in parts of an inch, the interval *v* being constantly 0·62 of an inch.

	Smallest.	Greatest.	Mean.
Air, *s* and S, pos.	0·60	0·79	0·695
Air, *s* and S, neg.	0·59	0·68	0·635
Oxygen, *s* and S, pos.	0·41	0·60	0·505
Oxygen, *s* and S, neg.	0·50	0·52	0·510
Nitrogen, *s* and S, pos.	0·55	0·68	0·615
Nitrogen, *s* and S, neg.	0·59	0·70	0·645
Hydrogen, *s* and S, pos.	0·30	0·44	0·370
Hydrogen, *s* and S, neg.	0·25	0·30	0·275
Carbonic acid, *s* and S, pos.	0·56	0 72	0·640
Carbonic acid, *s* and S, neg.	0·58	0·60	0·590
Olefiant gas, *s* and S, pos.	0·64	0·86	0·750
Olefiant gas, *s* and S, neg.	0·69	0·77	0·730
Coal gas, *s* and S, pos.	0·37	0·61	0·490
Coal gas, *s* and S, neg.	0·47	0·58	0·525
Muriatic acid gas, *s* and S, pos.	0·89	1·32	1·105
Muriatic acid gas, *s* and S, neg.	0·67	0·75	0·720

1389. The above results were all obtained at one time. On other occasions other experiments were made, which gave generally the same results as to order, though not as to numbers. Thus:

Hydrogen, *s* and S, pos.	0·23	0·57	0·400
Carbonic acid, *s* and S, pos.	0·51	1·05	0·780
Olefiant gas, *s* and S, pos.	0·66	1·27	0·965

I did not notice the difference of the barometer on the days of experiment.

1390. One would have expected only two distances, one for each interval, for which

the discharge might happen either at one or the other; and that the least alteration of either would immediately cause one to predominate constantly over the other. But that under common circumstances is not the case. With air in the receiver, the variation amounted to 0·2 of an inch nearly on the smaller interval of 0·6, and with muriatic acid gas, the variation was above 0·4 on the smaller interval of 0·9. Why is it that when a fixed interval (the one in the receiver) will pass a spark that cannot go across 0·6 of air at one time, it will immediately after, and apparently under exactly similar circumstances, not pass a spark that can go across 0·8 of air?

1391. It is probable that part of this variation will be traced to particles of dust in the air drawn into and about the circuit. I believe also that part depends upon a variable charged condition of the surface of the glass vessel *a*. That the whole of the effect is not traceable to the influence of circumstances in the vessel *a*, may be deduced from the fact, that when sparks occur between balls in free air they frequently are not straight, and often pass otherwise than by the shortest distance. These variations in air itself, and at different parts of the very same balls, show the presence and influence of circumstances which are calculated to produce effects of the kind now under consideration.

1392. When a spark had passed at either interval, then, generally, more tended to appear at the *same* interval, as if a preparation had been made for the passing of the latter sparks. So also on continuing to work the machine quickly the sparks generally followed at the same place. This effect is probably due in part to the warmth of the air heated by the preceding spark, in part to dust, and I suspect in part to something unperceived as yet in the circumstances of discharge.

1393. A very remarkable difference, which is *constant* in its direction, occurs when the electricity communicated to the balls *s* and S is changed from positive to negative, or in the contrary direction. It is that the range of variation is always greater when the small balls are positive than when they are negative. This is exhibited in the following Table, drawn from the former experiments.

	Pos.	Neg.
In Air the range was . .	0·19	0·09
Oxygen	0·19	0·02
Nitrogen	0·13	0·11
Hydrogen	0·14	0·05
Carbonic acid	0·16	0·02
Olefiant gas	0·22	0·08
Coal gas	0·24	0·12
Muriatic acid	0·43	0·08

I have no doubt these numbers require considerable correction, but the general result is striking, and the differences in several cases very great.

1394. Though, in consequence of the variation of the striking distance (1386.), the interval in air fails to be a measure, as yet, of the insulating or resisting power of the

gas in the vessel, yet we may for present purposes take the mean interval as representing in some degree that power. On examining these mean intervals as they are given in the third column (1388.), it will be very evident, that gases, when employed as dielectrics, have peculiar electrical relations to insulation, and therefore to induction, very distinct from such as might be supposed to depend upon their mere physical qualities of specific gravity or pressure.

1395. First, it is clear that at the *same pressure* they are not alike, the difference being as great as 37 and 110. When the small balls are charged positively, and with the same surfaces and the same pressure, muriatic acid gas has three times the insulating or restraining power (1362.) of hydrogen gas, and nearly twice that of oxygen, nitrogen, or air.

1396. Yet it is evident that the difference is not due to specific gravity, for though hydrogen is the lowest, and therefore lower than oxygen, oxygen is much beneath nitrogen, or than olefiant gas; and carbonic acid gas, though considerably heavier than olefiant gas or muriatic gas, is lower than either. Oxygen as a heavy, and olefiant as a light gas, are in strong contrast with each other; and if we may reason of olefiant gas from Harris's results with air (1365.), then it might be rarefied to two-thirds its usual density, or to a specific gravity of 9·3 (hydrogen being 1), and having neither the same density nor pressure as oxygen, would have equal insulating powers with it, or equal tendency to resist discharge.

1397. Experiments have already been described (1291. 1292.) which show that the gases are sensibly alike in their inductive capacity. This result is not in contradiction with the existence of great differences in their restraining power. The same point has been observed already in regard to dense and rare air (1375.).

1398. Hence arises a new argument proving that it cannot be mere pressure of the atmosphere which prevents or governs discharge (1377. 1378.), but a specific electric quality or relation of the gaseous medium. Hence also additional argument for the theory of molecular inductive action.

1399. Other specific differences amongst the gases may be drawn from the preceding series of experiments, rough and hasty as they are. Thus the positive and negative series of mean intervals do not give the same differences. It has already noticed that the negative numbers are lower than the positive (1393.), but, besides that, the *order* of the positive and negative results is not the same. Thus on comparing the mean numbers (which represent for the present insulating tension,) it appears that in air, hydrogen, carbonic acid, olefiant gas and muriatic acid, the tension rose higher when the smaller ball was made positive than when rendered negative, whilst in oxygen, nitrogen, and coal gas the reverse was the case. Now though the numbers cannot be trusted as exact, and though air, oxygen, and nitrogen should probably be on the same side, yet some of the results, as, for instance, those with muriatic acid, fully show a peculiar relation and difference amongst gases in this respect. This was further proved by making the interval in air 0·8 of an inch whilst

muriatic acid gas was in the vessel *a*; for on charging the small balls *s* and S positively, *all* the discharge took place through the *air*; but on charging them negatively, *all* the discharge took place through the *muriatic acid gas.*

1400. So also, when the conductor *n* was connected *only* with the muriatic acid gas apparatus, it was found that the discharge was more facile when the small ball *s* was negative than when positive; for in the latter case, much of the electricity passed off as brush discharge through the air from the connecting wire *p*; but in the former case, it all seemed to go through the muriatic acid.

1401. The consideration, however, of positive and negative discharge across air and other gases will be resumed in the further part of this, or in the next paper.

1402. Here for the present I must leave this part of the subject, which had for its object only to observe how far gases agreed or differed as to their power of retaining a charge on bodies acting by induction through them. All the results conspire to show that Induction is an action of contiguous molecules (1295. &c.); but besides confirming this, the first principle placed for proof in the present inquiry, they greatly assist in developing the specific properties of each gaseous dielectric, at the same time showing that further and extensive experimental investigation is necessary, and holding out the promise of new discovery as the reward of the labour required.

1403. When we pass from the consideration of dielectrics like the gases to that of bodies having the liquid and solid condition, then our reasonings in the present state of the subject assume much more of the character of mere supposition. Still I do not perceive anything adverse to the theory in the phenomena which such bodies present. If we take three insulating dielectrics, as air, oil of turpentine and shell-lac, and use the same balls or conductors at the same intervals in these three substances, increasing the intensity of the induction until discharge take place, we shall find that it must be raised much higher in the fluid than for the gas, and higher still in the solid than for the fluid. Nor is this inconsistent with the theory; for with the liquid, though its molecules are free to move almost as easily as those of the gas, there are many more particles introduced into the given interval; and as respects the latter circumstance, the same is the case when the solid body is employed. Besides that, the cohesive force of the body used will produce some effect; for though the production of the polarized states in the particle of a solid may not be obstructed, but, on the contrary, may in some cases be even favoured (1164. 1344.) by its solidity or other circumstances, yet solidity may well exert an influence on the point of its final subversion, (just as it prevents discharge in an electrolyte,) and so enable inductive intensity to rise to a much higher degree.

1404. In the cases of solids and liquids too, bodies may, and most probably do, possess specific differences as to their ability of assuming the polarized state, and

also as to the extent to which that polarity must rise before discharge occurs. An analogous difference exists in the specific inductive capacities already pointed out in a few substances (1278.) in the last paper. Such a difference might even account for the various degrees of insulating and conducting power possessed by different bodies, and, if it should be found to exist, would add further strength to the argument in favour of the molecular theory of inductive action.

1405. Having considered these various cases of sustained insulation in non-conducting dielectrics up to the highest point which they can attain, we find that they terminate at last in *disruptive discharge*; the peculiar condition of the molecules of the dielectric which was necessary to the continuous induction, being equally essential to the occurrence of that effect which closes all the phenomena. This discharge is not only in its appearance and condition different to the former modes by which the lowering of the powers was effected (1320. 1343.), but, whilst really the same in principle, varies much from itself in certain characters, and thus presents us with the forms of *spark*, *brush*, and *glow* (1359.). I will first consider *the spark*, limiting it for the present to the case of discharge between two oppositely electrified conducting surfaces.

The electric spark or flash.

1406. The *spark* is a discharge or lowering of the polarized inductive state of many dielectric particles, by a particular action of a few of the particles occupying a very small and limited space; all the previously polarized particles returning to their first or normal condition in the inverse order in which they left it, and uniting their powers meanwhile to produce, or rather to continue, (1417 and 1436.) the discharge effect in the place where the subversion of force first occurred. My impression is, that the few particles situated where discharge occurs are not merely pushed apart, but assume a peculiar state, a highly exalted condition for the time, i. e. have thrown upon them all the surrounding forces in succession, and rising up to a proportionate intensity of condition, perhaps equal to that of chemically combining atoms, discharge the powers, possibly in the same manner as they do theirs, by some operation at present unknown to us; and so the end of the whole. The ultimate effect is exactly as if a metallic wire had been put into the place of the discharging particles; and it does not seem impossible that the principles of action in both cases may, hereafter, prove to be the same.

1407. The *path of the spark*, or of the discharge, depends on the degree of tension acquired by the particles in the line of discharge, circumstances, which in every common case are very evident and by the theory easy to understand, rendering it higher in them than in their neighbours, and, by exalting them first to the requisite condition, causing them to determine the course of the discharge. Hence the se-

lection of the path, and the solution of the wonder which Harris has so well described* as existing under the old theory. All is prepared amongst the molecules beforehand, by the prior induction, for the path either of the electric spark or of lightning itself.

1408. The same difficulty is expressed as a principle by Nobili for voltaic electricity, almost in Mr. Harris's words, namely†, "electricity directs itself towards the point where it can most easily discharge itself," and the results of this as a principle he has well wrought out for the case of voltaic currents. But the *solution* of the difficulty, or the proximate cause of the effects, is the same: induction brings the particles up to or towards a certain state (1370.); and by those which first attain it, is the discharge first and most efficiently performed.

1409. The *moment* of discharge is probably determined by that molecule of the dielectric which, from the circumstances, has its tension most quickly raised up to the maximum intensity. In all cases where the discharge passes from conductor to conductor this molecule must be on the surface of one of them; but when it passes between a conductor and a non-conductor, it is, perhaps, not always so (1453.). When this particle has acquired its maximum tension, then the whole barrier of resistance is broken down in the line or lines of inductive action originating at it, and disruptive discharge occurs (1370.): and such an inference, drawn as it is from the theory, seems to me in accordance with Mr. Harris's facts and conclusions respecting the resistance of the atmosphere, namely, that it is not really greater at any one discharging distance than another‡.

1410. It seems probable, that the tension of a particle of the same dielectric, as air, which is requisite to produce discharge, is a *constant quantity*, whatever the shape of the part of the conductor with which it is in contact, whether ball or point; whatever the thickness or depth of dielectric throughout which induction is exerted; perhaps, even, whatever the state, as to rarefaction or condensation of the dielectric; and whatever the nature of the conductor, good or bad, with which the particle is for the moment associated. In saying so much, I do not mean to exclude small differences which may be caused by the reaction of neighbouring particles on the deciding particle, and indeed, it is evident that the intensity required in a particle must be related to the condition of those which are contiguous. But if the expectation should be found to approximate to truth, what a generality of character it presents! and, in the definiteness of the power possessed by a particular molecule, may we not hope to find an immediate relation to the force which, being electrical, is equally definite and constitutes chemical affinity?

1411. Theoretically it would seem that, at the moment of discharge by the spark in one line of inductive force, not merely would all the other lines throw their forces into this one (1406.), but the lateral effect, equivalent to a repulsion of these lines

* Nautical Magazine, 1834, p. 229. † Bibliotheque Universelle, 1835, lix. 275.

‡ Philosophical Transactions, 1834, pp. 227, 229.

(1224. 1297.), would be relieved and, perhaps, followed by something equivalent to a contrary action, amounting to a collapse or attraction of these parts. Having long sought for some transverse force in statical electricity, which should be the equivalent to magnetism or the transverse force of current electricity, and conceiving that it might be connected with the transverse action of the lines of inductive force already described (1297.), I was desirous, by various experiments, of bringing out the effect of such a force, and making it bear upon the phenomena of electro-magnetism and magneto-electricity.

1412. Amongst other results, I expected and sought for the mutual affection, or even the lateral coalition of two similar sparks, if they could be obtained simultaneously side by side, and sufficiently near to each other. For this purpose, two similar Leyden jars were supplied with rods of copper projecting from their balls in a horizontal direction, the rods being about 0·2 of an inch thick, and rounded at the ends. The jars were placed upon a sheet of tinfoil, and so adjusted that their rods, *a* and *b*, were near together, in the position represented in plan at fig. 2. *c* and *d* were two brass balls connected by a brass rod and insulated: *e* was also a brass ball connected, by a wire, with the ground and with the tinfoil upon which the Leyden jars were placed. By laying an insulated metal rod across from *a* to *b*, charging the jars, and removing the rod, both the jars could be brought up to the same intensity of charge (1370.). Then, making the ball *e* approach the ball *d*, at the moment the spark passed there, two sparks passed between the rods *n*, *o*, and the ball *c*; and as far as the eye could judge, or the conditions determine, they were simultaneous.

1413. Under these circumstances two modes of discharge took place; either each end had its own particular spark to the ball, or else one end only was associated by a spark with the ball, but was at the same time related to the other end by a spark between the two.

1414. When the ball *c* was about an inch in diameter, the ends *n* and *o*, about half an inch from it, and about 0·4 of an inch from each other, the two sparks to the ball could be obtained. When, for the purpose of bringing the sparks nearer together, the ends, *n* and *o*, were brought closer to each other, then, unless very carefully adjusted, only one end had a spark with the ball, the other having a spark to it; and the least variation of position would cause either *n* or *o* to be the end which, giving the direct spark to the ball, was also the one through, or by means of which, the other discharged its electricity.

1415. On making the ball *c* smaller, I found that then it was needful to make the interval between the ends *n* and *o* larger in proportion to the distance between them and the ball *c*. On making *c* larger, I found I could diminish the interval, and so bring the two simultaneous separate sparks closer together, until, at last, the distance between them was not more at the widest part than 0·6 of their whole length.

1416. Numerous sparks were then passed and carefully observed. They were very rarely straight, but either curved or bent irregularly. In the average of cases they were, I think, decidedly convex towards each other; perhaps two thirds presented more or less of this effect, the rest bulging more or less outwards. I was never able, however, to obtain sparks which, separately leaving the ends of the wires *n* and *o*, conjoined into one spark before they reached or communicated with the ball *c*. At present, therefore, though I think I saw a tendency in the sparks to unite, I cannot assert it as a fact.

1417. But there is one very interesting effect here analogous to, and it may be in part the same with, that I was searching for: I mean the increased facility of discharge where the spark passes. For instance, in the cases where one end, as *n*, discharged the electricity of both ends to the ball *c*, fig. 2., the electricity of the other end *o*, had to pass through an interval of air 1·5 times as great as that which it might have taken, by its direct passage between the end and the ball itself. In such cases, the eye could not distinguish, even by the use of Wheatstone's means*, that the spark from the end *n*, which contained both portions of electricity, was a double spark. It could not have consisted of two sparks taking separate courses, for such an effect would have been visible to the eye; but it is just possible, that the spark of the first end *n* and its jar, passing at the smallest interval of time before that of the other *o*, had heated and expanded the air in its course, and made it so much more favourable to discharge, that the electricity of the end *o* preferred leaping across to it and taking a very circuitous route, rather than the more direct one to the ball. It must, however, be remarked, in answer to this supposition, that the one spark between *d* and *e* would, by its influence, tend to produce simultaneous discharges at *n* and *o*, and certainly did so, when no preponderance was given to one wire over the other, as to the previous inductive effect (1414.).

1418. The fact, however, is, that disruptive discharge is favourable to itself. It is at the outset a case of tottering equilibrium: and if *time* be an element in discharge, in however minute a proportion (1436.), then the commencement of the act at any point favours its continuance and increase there, and portions of power will be discharged by a course which they would not otherwise have taken.

1419. The mere heating and expansion of the air itself by the first portion of electricity which passes, must have a great influence in producing this result.

1420. As to the result itself, we see its influence in every spark that passes; for it is not the whole quantity which passes that determines the discharge, but merely that small portion of force which brings the deciding molecule (1370.) up to its maximum tension; then, when its forces are subverted and discharge begins, all the rest passes by the same course, from the influence of the favouring circumstances just referred to; and whether it be the electricity on a square inch, or a thousand

* Philosophical Transactions, 1834, pp. 584, 585.

square inches of charged glass, the discharge is complete. Hereafter we shall find the influence of this effect in the formation of brushes (1435.); and it is not impossible that we may trace it producing the jagged spark and the forked lightning.

1421. The characters of the electric spark in *different gases* vary, and the variation *may* be due simply to the effect of the heat evolved at the moment. But it may also be due to that specific relation of the particles and the electric forces which I have assumed as the basis of a theory of induction; the facts do not oppose such a view; and in that view, the variation strengthens the argument for molecular action, as it would seem to show the influence of the latter in every part of the electrical effect (1423. 1454.).

1422. The appearances of the sparks in different gases have often been observed and recorded*, but I think it not out of place to notice briefly the following results; they were obtained with balls of brass, (platina surfaces would have been better,) and at common pressures. In *air*, the sparks have that intense light and bluish colour which are so well known, and often have faint or dark parts in their course, when the quantity of electricity passing is not great. In *nitrogen*, they are very beautiful, having the same general appearance as in air, but have decidedly more colour of a bluish or purple character, and I thought were remarkably sonorous. In *oxygen*, the sparks were whiter than in air or nitrogen, and I think not so brilliant. In *hydrogen*, they had a very fine crimson colour, not due to its rarity, for the character passed away as the atmosphere was rarefied (1459.)†. Very little sound was produced in this gas; but that is a consequence of its physical condition‡. In *carbonic acid gas*, the colour was similar to that of the spark in air, but with a little green in it: the sparks were remarkably irregular in form, more so than in common air: they could also, under similar circumstances as to size of ball, &c., be obtained much longer than in air, the gas showing a singular readiness to pass the discharge in the form of spark. In *muriatic acid gas*, the spark was nearly white: it was always bright throughout, never presenting those dark parts which happen in air, nitrogen, and some other gases. The gas was dry, and during the whole experiment the surface of the glass globe within remained quite dry and bright. In *coal gas*, the spark was sometimes green, sometimes red, and occasionally one part was green and another red. Black parts also occur very suddenly in the line of the spark, i. e. they are not connected by any dull part with bright portions, but the two seem to join directly one with the other.

1423. These varieties of character impress my mind with a feeling, that they are due to a direct relation of the electric powers to the particles of the dielectric through which the discharge occurs, and are not the mere results of a casual ignition or a

* See Van Marum's description of the Teylerian machine, vol. i. p. 112., and vol. ii. p. 196.; also Ency. Britan., vol. vi., Article Electricity, pp. 505, 507.

† Van Marum says they are about four times as large in hydrogen as in air, vol. i. p. 122.

‡ Leslie.

secondary kind of action of the electricity, upon the particles which it finds in its course and thrusts aside in its passage (1454.).

1424. The spark may be obtained in media which are far denser than air, as in oil of turpentine, olive oil, resin, glass, &c.: it may also be obtained in bodies which being denser likewise approximate to the condition of conductors, as spermaceti, water, &c. But in these cases, nothing occurs which, as far as I can perceive, is at all hostile to the general views I have endeavoured to advocate.

The electrical brush.

1425. The *brush* is the next form of disruptive discharge which I will consider. There are many ways of obtaining it, or rather of exalting its characters; and all these ways illustrate the principles upon which it is produced. If an insulated conductor, connected with the positive conductor of an electrical machine, have a metal rod 0·3 of an inch in diameter projecting from it outwards from the machine, and terminating by a rounded end or a small ball, it will generally give good brushes; or, if the machine be not in good action, then many ways of assisting the formation of the brush can be resorted to; thus, the hand or any *large* conducting surface may be approached towards the termination to increase inductive force (1374.): or the termination may be smaller and of badly conducting matter, as wood: or sparks may be taken between the prime conductor of the machine and the secondary conductor to which the termination giving brushes belongs: or, which gives to the brushes exceedingly fine characters and great magnitude, the air around the termination may be rarefied more or less, either by heat or the air pump; the former favourable circumstances being also continued.

1426. The brush when obtained by a powerful machine on a ball about 0·7 of an inch in diameter, at the end of a long brass rod attached to the positive prime conductor, had the general appearance as to form represented in fig. 3.: a short conical bright part or root appeared at the middle part of the ball projecting directly from it, which, at a little distance from the ball, broke out suddenly into a wide brush of pale ramifications having a quivering motion, and being accompanied at the same time with a low dull chattering sound.

1427. At first the brush seems continuous, but Professor Wheatstone has shown that the whole phenomenon consists of successive intermitting discharges*. If the eye be passed rapidly, not by a motion of the head, but of the eyeball itself, across the direction of the brush, by first looking steadfastly about 10° or 15° above, and then instantly as much below it, the general brush will be resolved into a number of individual brushes, standing in a row upon the line which the eye passed over; each elementary brush being the result of a single discharge, and the space between them representing both the time during which the eye was passing over that space, and that which elapsed between one discharge and another.

* Philosophical Transactions, 1834, p. 586.

1428. The single brushes could easily be separated to eight or ten times their own width, but were not at the same time extended, i. e. they did not become more indefinite in shape, but, on the contrary, less so, each being more distinct in form, ramification, and character, because of its separation from the others, in its effects upon the eye. Each, therefore, was instantaneous in its existence (1436.). Each had the conical root complete (1426.).

1429. On using a smaller ball, the general brush was smaller, and the sound, though weaker, more continuous. On resolving the brush into its elementary parts, as before, these were found to occur at much shorter intervals than in the former case, but still the discharge was intermitting.

1430. Employing a wire with a round end, the brush was still smaller, but, as before, separable into successive discharges. The sound, though feebler, was higher in pitch, being a distinct musical note.

1431. The sound is, in fact, due to the recurrence of the noise of each separate discharge, and these, happening at intervals nearly equal under ordinary circumstances, cause a definite note to be heard, which, rising in pitch with the increased rapidity and regularity of the intermitting discharges, gives a ready and accurate measure of the intervals, and so may be used in any case when the discharge is heard, even though the appearances may not be seen, to determine the element of *time*. So also, when, by bringing the hand towards a projecting rod or ball, the pitch of the tone produced by a brushy discharge increases, the effect informs us that we have increased the induction (1374.), and by that means increased the rapidity of the alternations of charge and discharge.

1432. By using wires with finer terminations, smaller brushes were obtained, until they could hardly be distinguished as brushes; but as long as *sound* was heard, the discharge could be ascertained by the eye to be intermitting; and when the sound ceased, the light became *continuous* as a glow (1359. 1405.).

1433. To those not accustomed to use the eye in the manner I have described, or, in cases where the recurrence is too quick for any unassisted eye, the beautiful revolving mirror of Professor Wheatstone* will be useful for such developments of condition as those mentioned above. Another excellent process is to produce the brush or other luminous phenomenon on the end of a rod held in the hand opposite to a charged positive or negative conductor, and then move the rod rapidly from side to side whilst the eye remains still. The successive discharges occur of course in different places, and the state of things before, at, and after a single coruscation or brush can be exceedingly well separated.

1434. The *brush* is in reality a discharge between a bad or a non-conductor and either a conductor or another non-conductor. Under common circumstances, the brush is a discharge between a conductor and air, and I conceive it to take place in something like the following manner. When the end of an electrified rod projects into

* Philosophical Transactions, 1834, pp. 584, 585.

the middle of a room, induction takes place between it and the walls of the room, across the dielectric, air; and the lines of inductive force accumulate upon the end in greater quantity than elsewhere, or the particles of air at the end of the rod are more highly polarized than those at any other part of the rod, for the reasons already given (1374.). The particles of air situated in sections across these lines of force are least polarized in sections towards the walls, and most polarized in those nearer to the end of the wires (1369.): thus, it may well happen, that a particle at the end of the wire is at a tension that will immediately terminate in discharge, whilst in those even only a few inches off, the tension is still beneath that point. But suppose the rod to be charged positively, a particle of air A, fig. 4. next it, being polarized, and having of course its negative force directed towards the rod and its positive force outwards; the instant that discharge takes place between the positive force of the particle of the rod opposite the air and the negative force of the particle of air towards the rod, the whole particle of air becomes positively electrified; and when, the next instant, the discharged part of the rod resumes its positive state, by conduction from the surface of metal behind, it not only acts on the particles beyond A, by throwing A into a polarized state again, but A itself, because of its charged state, exerts a distinct inductive act towards these further particles, and the tension is consequently so much exalted between A and B, that discharge takes place there also, as well as again between the metal and A.

1435. In addition to this effect, it has been shown, that, the act of discharge having once commenced, the whole operation, like a case of unstable equilibrium, is hastened to a conclusion (1370. 1418.), the rest of the act being facilitated in its occurrence, and other electricity than that which caused the first necessary tension hurrying to the spot. When, therefore, disruptive discharge has once commenced at the root of a brush, the electric force which has been accumulating in the conductor attached to the rod, finds a more ready discharge there than elsewhere, and will at once follow the course marked out as it were for it, thus leaving the conductor in a partially discharged state, and the air about the end of the wire in a charged condition; and the time necessary for restoring the full charge of the conductor, and the dispersion of the charged air in a greater or smaller degree, by the joint forces of repulsion from the conductor and attraction towards the walls of the room, to which its inductive action is directed, is just that time which forms the interval between brush and brush (1420. 1427. 1431.).

1436. The words of this description are long, but there is nothing in the act or the forces on which it depends to prevent its being *instantaneous*, as far as we can estimate and measure it. The consideration of *time* is, however, important in several points of view (1418.), and in reference to disruptive discharge, it seemed from theory far more probable that it might be detected in a brush than in a spark, for in a brush, the particles in the line through which the discharge passes are in very different states as to intensity, and the discharge is already complete in its act at the root of

the brush, before the particles at the extremity of the ramifications have yet attained their maximum intensity.

1437. I consider brush discharge as, probably, a successive effect in this way. Discharge begins at the root (1426.), and, extending itself in succession to all parts of the single brush, continues to go on at the root and the previously formed parts until the whole brush is complete; then, by the fall in intensity and power at the conductor, it ceases at once in all parts, to be renewed, when that power has risen again to a sufficient degree. But in a spark, the particles in the line of discharge being, from the circumstances, nearly alike in their intensity of polarization, suffer discharge so nearly at the same moment as to make the time quite insensible to us.

1438. Mr. Wheatstone has already made experiments which fully illustrate this point. He found that the brush generally had a sensible duration, but that with his highest capabilities he could not detect any such effect in the spark*. I repeated his experiment on the brush, though with more imperfect means, to ascertain whether I could distinguish a longer duration in the stem or root of the brush than in the extremities, and the appearances were such as to make me think an effect of this kind was produced.

1439. That the discharge breaks into several ramifications, and by them passes through portions of air alike, or nearly alike, as to polarization and the degree of tension the particles there have acquired, is a very natural result of the previous state of things, and sooner to be expected than that the discharge should continue to go straight out into space in a single line amongst those particles which, being at a distance from the end of the rod, are in a lower state of tension than those which are near: and whilst we cannot but conclude, that those parts where the branches of a single brush appear, are more favourably circumstanced for discharge than the darker parts between the ramifications, we may also conclude, that in those parts where the light of concomitant discharge is equal, there the circumstances are nearly equal also. The single brushes are by no means of the same particular shape even when they are observed without displacement of the rod or surrounding objects (1427. 1433.), and the successive discharges may be considered as taking place into the mass of air around, through different roads at each brush, according as minute circumstances, as dust, &c. (1391. 1392.) may have favoured the course by one set of particles rather than another.

1440. Brush discharge does not essentially require any current of the medium in which the brush appears: the current almost always occurs, but is a consequence of the brush, and will be considered hereafter. On holding a blunt point positively charged towards uninsulated water, a star or glow appeared on the point, a current of air passed from it, and the surface of the water was depressed; but on bringing

* Philosophical Transactions, 1836, pp. 586, 590.

the point so near that sonorous brushes passed, then the current of air instantly ceased, and the surface of the water became level.

1441. The discharge by a brush is not to all the particles of air that are near the electrified conductor from which the brush issues; only those parts where the ramifications pass are electrified: the air in the central dark parts between them receives no charge, and, in fact, at the time of discharge, has its electric and inductive tension considerably lowered. For consider fig. 14. to represent a single positive brush;—the induction before the discharge is from the end of the rod outwards, in diverging lines towards the distant conductors, as the walls of the room, &c., and a particle at *a* has polarity of a certain degree of tension, and tends with a certain force to become charged; but at the moment of discharge, the air in the ramifications *b* and *d*, acquiring also a positive state, opposes its influence to that of the positive conductor on *a*, and the tension of the particle at *a* is therefore diminished rather than increased. The charged particles at *b* and *d* are now inductive bodies, but their lines of inductive action are still outwards towards the walls of the room; the direction of the polarity and the tendency of other particles to charge from these, being governed by, or in conformity with, these lines of force.

1442. The particles that are charged are probably very highly charged, but, the medium being a non-conductor, they cannot communicate that state to their neighbours. They travel, therefore, under the influence of the repulsive and attractive forces, from the charged conductor towards the nearest uninsulated conductor, or the nearest body in a different state to themselves, just as charged particles of dust would travel, and are then discharged; each particle acting, in its course, as a centre of inductive force upon any bodies near which it may come.

1443. The travelling of these charged particles when they are numerous, causes wind and currents, but these will come into consideration under *carrying discharge* (1319.). When air is said to be electrified, and it frequently assumes this state near electrical machines, it consists, according to my view, of a mixture of electrified and unelectrified particles, the latter being in very large proportion to the former. When we gather electricity from air by a flame or by wires, it is either by the actual discharge of these particles, or by effects dependent on their inductive action, a case of either kind being produceable at pleasure. That the law of equality between the two forces or forms of force in inductive action is as strictly preserved in these as in other cases, is fully shown by the fact, formerly stated (1173. 1174.), that, however strongly air in a vessel might be charged positively, there was an exactly equal amount of negative force on the inner surface of the vessel itself, for no residual portion of either the one or the other electricity could be obtained.

1444. I have nowhere said, nor does it follow, that the air is charged only where the luminous brush appears. The charging may extend beyond those parts which are visible, i. e. particles to the right or left of the lines of light may receive electri-

city, the parts which are luminous being so only because much electricity is passing by them to other parts (1437.); just as in a spark discharge the light is greater as more electricity passes, though it has no necessary relation to the quantity required to commence discharge (1370. 1420.). Hence the form we see in a brush may by no means represent the whole quantity of air electrified; for an invisible portion, clothing the visible form to a certain depth, may, at the same time, receive its charge.

1445. Several effects which I have met with in muriatic acid gas tend to make me believe, that that gaseous body allows of a dark discharge. At the same time, it is quite clear from theory, that in some gases, the reverse of this may occur, i. e. that the charging of the air may not extend even so far as the light. We do not know as yet enough of the electric light to be able to state on what it depends, and it is very possible that, when electricity bursts forth into air, all the particles of which are in a state of tension, light may be evolved by such as, being very near to, are not of, those which actually receive a charge at the time.

1446. The further a brush extends in a gas, the further no doubt is the charge or discharge carried forward; but this may vary between different gases, and yet the intensity required for the first moment of discharge not vary in the same, but in some other proportion. Thus with respect to nitrogen and muriatic acid gases, the former, as far as my experiments have proceeded, produces far finer and larger brushes than the latter (1458. 1462.), but the intensity required to commence discharge is much higher for the latter than the former (1395.). Here again, therefore, as in many other qualities, specific differences are presented by different gaseous dielectrics, and so prove the special relation of the latter to the act and the phenomena of induction.

1447. To sum up these considerations respecting the character and condition of the brush, I may state that it is a spark to air; a diffusion of electric force to matter, not by conduction, but disruptive discharge; a dilute spark which, passing to very badly conducting matter, frequently discharges but a small portion of the power stored up in the conductor; for as the air charged reacts on the conductor, whilst the conductor, by loss of electricity, sinks in its force, the discharge quickly ceases, until by the dispersion of the charged air and the renewal of the excited conditions of the conductor, circumstances have risen up to their first effective condition, again to cause discharge, and again to fall and rise.

1448. The brush and spark gradually pass into one another. Making a small ball positive by a good electrical machine with a large prime conductor, and approaching a large uninsulated discharging ball towards it, very beautiful variations from the spark to the brush may be obtained. The drawings of long and powerful sparks, given by VAN MARUM*, HARRIS†, and others, also indicate the same phenomena. As far as I have observed, whenever the spark has been brushy in air of common press-

* Description of the TEYLERIAN machine, vol. i. pp. 28. 32.; vol. ii. p. 226, &c.

† Philosophical Transactions, 1834, p. 243.

ures, the whole of the electricity has not been discharged, but only portions of it, more or less according to circumstances: whereas, whenever the effect has been a distinct spark throughout the whole of its course, the discharge has been perfect, provided no interruption had been made to it elsewhere, in the discharging circuit, than where the spark occurred.

1449. When an electrical brush from an inch to six inches in length or more is issuing into free air, it has the form given, fig. 3. But if the hand, a ball, or any knobbed conductor be brought near, the extremities of the coruscations turn towards it and each other, and the whole assumes various forms according to circumstances, as in figs. 5, 6, and 7. The influence of the circumstances in each case is easily traced, and I might describe it here, but that I should be ashamed to occupy the time of the Society in things so evident. But how beautifully does the curvature of the ramifications illustrate the curved form of the lines of inductive force existing previous to the discharge! for the former are consequences of the latter, and take their course, in each discharge, where the previous inductive tension had been raised to the proper degree. They represent these curves just as well as iron filings represent magnetic curves, the visible effects in both cases being the consequences of the action of the forces in *the places where* the effects appear. The phenomena, therefore, constitute additional and powerful testimony (1216. 1230.) to that already given in favour both of induction through dielectrics in curved lines (1231.), and of the lateral relation of these lines, by an effect equivalent to a repulsion producing divergence, or, as in the cases figured, the bulging form.

1450. In reference to the theory of molecular inductive action, I may also add here, the proof deducible from the long brushy ramifying spark which may be obtained between a small ball on the positive conductor of an electrical machine, and a larger one at a distance (1448.). What a fine illustration that spark affords of the previous condition of *all* the particles of the dielectric between the surfaces of discharge, and how unlike the appearances are to any which would be deduced from the theory which assumes inductive action to be action at a distance, in straight lines only; and charge, as being electricity retained upon the surface of conductors by the mere pressure of the atmosphere!

1451. When the brush is obtained in rarefied air, the appearances vary greatly, according to circumstances, and are exceedingly beautiful. Sometimes a brush may be formed of only six or seven branches, these being broad and highly luminous, of a purple colour, and in some parts an inch or more apart:—by a spark discharge at the prime conductor (1455.) single brushes may be obtained at pleasure. Discharge in the form of a brush is favoured by rarefaction of the air, in the same manner and for the same reason as discharge in the form of a spark (1375.); but in every case

there is previous induction and charge through the dielectric, and polarity of its particles (1437.), the induction being, as in any other instance, alternately raised by the machine and lowered by the discharge. In certain experiments the rarefaction was increased to the utmost degree, and the opposed conducting surfaces brought as near together as possible without producing the glow: the brushes then contracted in their lateral dimensions, and recurred so rapidly as to form an apparently continuous arc of light from metal to metal. Still the discharge could be observed to intermit (1427.), so that even under these high conditions, induction preceded each single brush, and the tense polarized condition of the contiguous particles was a necessary preparation for the discharge itself.

1452. The brush form of disruptive discharge may be obtained not only in air and gases, but also in much denser media. I procured it in oil of turpentine from the end of a wire going through a glass tube into the fluid contained in a metal vessel. The brush was small and very difficult to obtain; the ramifications were simple, and stretched out from each other diverging very much. The light was exceedingly feeble, a perfectly dark room being required for its observation. When a few solid particles, as of dust or silk, were in the liquid, the brush was produced with much greater facility.

1453. The running together or coalescence of different lines of discharge (1412.) is very beautifully shown in the brush in air. This point may present a little difficulty to those who are not accustomed to see in every discharge an equal exertion of power in opposite directions, a positive brush being considered by such (perhaps in consequence of the common phrase *direction of a current*) as indicating a breaking forth in different directions of the original force, rather than a tendency to convergence and union in one line of passage. But the ordinary case of the brush may be compared, for its illustration, with that in which, by holding the knuckle opposite to highly excited glass, a discharge occurs, the ramifications of a brush then leading from the glass and converging into a spark on the knuckle. Though a difficult experiment to make, it is possible to obtain discharge between highly excited shell-lac and the excited glass of a machine: when the discharge passes, it is, from the nature of the charged bodies, brush at each end and spark in the middle, beautifully illustrating that tendency of discharge to facilitate like action, which I have described in a former page (1418.).

1454. The brush has *specific characters* in different gases, indicating a relation to the particles of these bodies even in a stronger degree than the spark (1422. 1423.). This effect is in strong contrast with the non-variation caused by the use of different substances as *conductors* from which the brushes are to originate. Thus, using such bodies as wood, card, charcoal, nitre, citric acid, oxalic acid, oxide of lead, chloride of lead, carbonate of potassa, potassa fusa, strong solution of potash, oil of vitriol, sulphur, sulphuret of antimony, and hæmatite, no variation in the character of the brushes was obtained, except that (dependent upon their effect as better or worse

conductors) of causing discharge with more or less readiness and quickness from the machine*.

1455. The following are a few of the effects I observed in different gases at the positively charged surfaces, and with atmospheres varying in their pressure. The general effect of rarefaction was the same for all the gases: at first, sparks passed; these gradually were converted into brushes, which became larger and more distinct in their ramifications, until, upon further rarefaction, the latter began to collapse and draw in upon each other, till they formed a stream across from conductor to conductor: then a few lateral streams shot out towards the glass of the vessel from the conductors; these became thick, flossy, and soft in appearance, and were succeeded by the full constant glow which covered the discharging wire. The phenomena varied with the size of the vessel (1477.), the degree of rarefaction, and the discharge of electricity from the machine. When the latter was in successive sparks, they were most beautiful, the effect of a spark from a small machine being equal to, and often surpassing, that produced by the *constant* discharge of a far more powerful one.

1456. *Air.*—Fine positive brushes are easily obtained in air at common pressures, and possess the well-known purplish light. When the air is rarefied, the ramifications are very long, filling the globe (1477.), the light is greatly increased, and is of a beautiful purple colour, with an occasional rose tint in it.

1457. *Oxygen.*—At common pressures, the brush is very close and compressed, and of a dull whitish colour. In rarefied oxygen, the form and appearance are better, the colour somewhat purplish, but all the characters very poor compared to those in air.

1458. *Nitrogen* gives brushes with great facility at the positive surface, far beyond any other gas I have tried: they are almost always fine in form, light, and colour, and in rarefied nitrogen are magnificent. They surpass the discharges in any other gas as to the quantity of light evolved.

1459. *Hydrogen,* at common pressures, gave a better brush than oxygen, but did not equal nitrogen; the colour was greenish grey. In rarefied hydrogen, the ramifications were very fine in form and distinctness, but pale in colour, with a soft and velvety appearance, and not at all equal to those in nitrogen. In the rarest state of the gas, the colour of the light was a pale gray green.

1460. *Coal gas.*—The brushes were rather difficult to produce, the contrast with nitrogen being great in this respect. They were short and strong, generally of a greenish colour, and possessing much of the spark character: for, occurring on both the positive and negative terminations, often when there was a dark interval of some length between the two brushes, still the quick, sharp sound of the spark was

* Exception must, of course, be made of those cases where the root of the brush, becoming a spark, causes a little diffusion or even decomposition of the matter there, and so gains more or less of a particular colour at that part.

produced, as if the discharge had been sudden through this gas, and partaking, in that respect, of the character of a spark. In rare coal gas, the forms were better, but the light very poor and the colour gray.

1461. *Carbonic acid gas* produces a very poor brush at common pressures, as regards either size, light, or colour; and this is probably connected with the tendency which this gas has to discharge the electricity as a spark (1422.). In rarefied carbonic acid, the brush is better in form, but weak as to light, being of a dull greenish or purplish hue, varying with the pressure and other circumstances.

1462. *Muriatic acid gas.*—It is very difficult to obtain the brush in this gas at common pressures. On gradually increasing the distance of the rounded ends, the sparks suddenly ceased when the interval was about an inch, and the discharge, which was still through the gas in the globe, was silent and dark. Occasionally a very short brush could for a few moments be obtained, but it quickly disappeared again. Even when the intermitting spark current (1455.) from the machine was used, still I could only with difficulty obtain a brush, and that very short, though I used rods with rounded terminations (about 0·25 of inch in diameter) which had before given them most freely in air and nitrogen. During the time of this difficulty with the muriatic gas, magnificent brushes were passing off from different parts of the machine into the surrounding air. On rarefying the gas, the formation of the brush was facilitated, but it was generally of a low squat form, very poor in light, and very similar on both the positive and negative surfaces. On rarefying the gas still more, a few large ramifications were obtained of a pale bluish colour, utterly unlike those in nitrogen.

1463. In all the gases, the different forms of disruptive discharge may be linked together and gradually traced from one extreme to the other, i. e. from the spark to the glow (1405.), or, it may be, to a still further condition to be called dark discharge; but it is, nevertheless, very surprising to see what a specific character each keeps whilst under the predominance of the general law. Thus, in muriatic acid, the brush is very difficult to obtain, and there comes in its place almost a dark discharge, partaking of the readiness of the spark action. Moreover, in muriatic acid, I have *never* observed the spark with any dark interval in it. In nitrogen, the spark readily changes its character into that of brush. In carbonic acid gas, there seems to be a facility to occasion spark discharge, whilst yet that gas is unlike nitrogen in the facility of the latter to form brushes, and unlike muriatic acid in its own facility to continue the spark. These differences add further force, first to the observations already made respecting the spark in various gases (1422. 1423.), and then, to the proofs deducible from it, of the relation of the electrical forces to the particles of matter.

1464. The peculiar characters of nitrogen in relation to the electric discharge (1422. 1458.) must, evidently, have an important influence over the form and even the occurrence of lightning. Being that gas which most readily produces corusca-

tions, and, by them, extends discharge to a greater distance than any other gas tried, it is also that which constitutes four fifths of our atmosphere; and as, in atmospheric electrical phenomena, one, and sometimes both the inductive forces are resident on the particles of the air, which, though probably affected as to conducting power by the aqueous particles in it, cannot be considered as a good conductor, so the peculiar power possessed by nitrogen, to originate and effect discharge in the form of a brush or of ramifications, has, probably, an important relation to its electrical service in nature, as it most seriously affects the character and condition of the discharge when made. The whole subject of discharge from and through gases is a most important one to science, and, if only in reference to atmospheric electricity, deserves extensive and close experimental investigation.

Difference of discharge at the positive and negative conducting surfaces.

1465. I have avoided speaking of this well-known phenomenon more than was quite necessary, that I might bring together here what I have to say on the subject. When the brush discharge is observed in air at the positive and negative surfaces, there is a very striking difference, the true and full comprehension of which would, no doubt, be of the utmost importance to the physics of electricity; it would throw great light on our present subject, i. e. the molecular action of dielectrics under induction, and its consequences, and seems very open to, and accessible by, experimental inquiry.

1466. The difference in question used to be expressed in former times by saying, that a point charged positively gave brushes into the air, whilst the same point charged negatively gave a star. This is true only of bad conductors, or of metallic conductors charged intermittingly, or otherwise controlled by collateral induction. If metallic points project *freely* into the air, the positive and negative light upon them differ very little in appearance, and the difference can be observed only upon close examination.

1467. The effect varies exceedingly under different circumstances, but, as we must set out from some position, may perhaps be stated thus: if a metallic wire with a rounded termination in free air be used to produce the brushy discharge, then the brushes obtained when the wire is charged negatively are very poor and small, by comparison with those produced when the charge is positive. Or if a large metal ball connected with the electrical machine be charged *positively*, and a fine uninsulated point be gradually brought towards it, a star appears on the point when at a considerable distance, which, though it becomes brighter, does not change its form of a star until it is close up to the ball: whereas, if the ball be charged negatively, the point at a considerable distance has a star on it as before; but when brought nearer, (in my case to the distance of $1\frac{1}{2}$ inches,) a brush formed on it, extending to the negative ball; and when still nearer, (at $\frac{1}{8}$ of an inch distance,) the brush ceased, and bright sparks passed. These variations, I believe, include the whole series of differ-

ences, and they seem to show at once, that the negative surface tends to retain its discharging character unchanged, whilst the positive surface, under similar circumstances, permits of great variation.

1468. There are several points in the character of the negative discharge to air which it is important to observe. A metal rod, 0·3 of an inch in diameter, with a rounded end projecting into the air, was charged negatively, and gave a short noisy brush (fig. 8.). It was ascertained both by sight (1427. 1433.) and sound (1431.), that the successive discharges were very rapid in their recurrence, being seven or eight times more numerous in the same period, than those produced when the rod was charged positively to an equal degree. When the rod was positive, it was easy, by working the machine a little quicker, to replace the brush by a glow (1405. 1463.), but when it was negative no efforts could produce this change. Even by bringing the hand opposite the wire, the only effect was to increase the number of brush discharges in a given period, raising at the same time the sound to a higher pitch.

1469. A point opposite the negative brush exhibited a star, and as it was approximated caused the size and sound of the negative brush to diminish, and, at last, to cease, leaving the negative end silent and dark, yet effective as to discharge.

1470. When the round end of a smaller wire (fig. 9.) was advanced towards the negative brush, it (becoming positive by induction) exhibited the quiet glow at 8 inches distance, the negative brush continuing. When nearer, the pitch of the sound of the negative brush rose, indicating quicker intermittences (1431.); still nearer, the positive end threw off ramifications and distinct brushes; at the same time, the negative brush contracted in its lateral directions and collected together, giving a peculiar narrow longish brush, in shape like a hair pencil, the two brushes existing at once, but very different in their form and appearance, and especially in the more rapid recurrence of the negative discharges than of the positive. On using a smaller positive wire for the same experiment, the glow first appeared on it, and then the brush, the negative brush being affected at the same time; and the two at one distance became exceedingly alike in appearance, and the sounds, I thought, were in unison; at all events they were in harmony, so that the intermissions of discharge were either isochronous, or a simple ratio existed between the intervals. With a higher action of the machine, the wires being retained unaltered, the negative surface would become dark and silent, and a glow appear on the positive one. A still higher action changed the latter into a spark. Finer positive wires gave other variations of these effects, which I must not allow myself to go into here.

1471. A thinner rod was now connected with the negative conductor in place of the larger one (1468.), its termination being gradually diminished to a blunt point, as in fig. 10.; and it was beautiful to observe that, notwithstanding the variation of the brush, the same general order of effects was produced. The end gave a small sonorous negative brush, which the approach of the hand or a large conducting surface did not alter, until it was so near as to produce a spark. A fine point oppo-

site to it was luminous at a distance; being nearer it did not destroy the light and sound of the negative brush, but only tended to have a brush produced on itself, which, at a still nearer distance, passed into a spark joining the two surfaces.

1472. When the distinct negative and positive brushes are produced simultaneously in relation to each other in air, the former almost always has a contracted form, as in fig. 11., very much indeed resembling the figure which the positive brush itself has when influenced by the lateral vicinity of positive parts acting by induction. Thus a brush issuing from a point in the re-entering angle of a positive conductor has the same compressed form (fig. 12.).

1473. The character of the negative brush is not affected by the chemical nature of the substances of the conductors (1454.), but only by their possession of the conducting power in a greater or smaller degree.

1474. Rarefaction of common air about a negative ball or blunt point facilitated the development of the negative brush, the effect being, I think, greater than on a positive brush, though great on both. Extensive ramifications could be obtained from a ball or end electrified negatively to the plate of the air-pump on which the jar containing it stood.

1475. A very important variation of the relative forms and conditions of the positive and negative brush takes place on varying the dielectric in which they are produced. The difference is so very great that it points to a specific relation of this form of discharge to the particular gas in which it takes place, and opposes the idea that gases are but obstructions to the discharge, acting one like another and merely in proportion to their pressure (1377.).

1476. In *air*, the superiority of the positive brush is well known (1467. 1472.). In *nitrogen*, it is as great or even greater than in air (1458.). In *hydrogen*, the positive brush loses a part of its superiority, not being so good as in nitrogen or air; whilst the negative brush does not seem injured (1459.). In *oxygen*, the positive brush is compressed and poor (1457.); whilst the negative did not sink in character: the two were so alike that the eye frequently could not tell the one from the other, and this similarity continued when the oxygen was gradually rarefied. In *coal gas* the brushes are difficult of production as compared to nitrogen (1460.), and the positive not much superior to the negative in its character, either at common or low pressures. *In carbonic acid gas*, this approximation of character also occurred. In *muriatic acid gas* the positive brush was very little better than the negative, and both difficult to produce (1462.) as compared with the facility in nitrogen or air.

1477. These experiments were made with rods of brass about a quarter of an inch thick having rounded ends, the ends being opposed in a glass globe 7 inches in diameter, containing the gas to be experimented with. The electric machine was used to communicate directly, sometimes the positive, and sometimes the negative, state, to the rod in connection with it.

1478. Thus we see that, notwithstanding there is a general difference in favour of the superiority of the positive brush over the negative, that difference is at its maximum in nitrogen and air; whilst in carbonic acid, muriatic acid, coal gas, and oxygen it diminishes, and at last becomes almost nothing. So that in this particular effect, as in all others yet examined, the evidence is in favour of that view which refers the results to a direct relation of the electric forces with the molecules of the matter concerned in the action (1421. 1423. 1463.). Even when special phenomena arise under the operation of the general law, the theory adopted seems fully competent to meet the case.

1479. Before I proceed further in tracing the probable cause of the difference between the positive and negative brush discharge, I wish to know the results of a few experiments which are in course of preparation: and thinking this Series of Researches long enough, I shall here close it with the expectation of being able in a few weeks to renew the inquiry, and entirely redeem my pledge (1306.).

Royal Institution,
Dec. 23*rd*, 1837.

VIII. *Experimental Researches in Electricity.—Thirteenth Series.* By Michael Faraday, *Esq., D.C.L. F.R.S. Fullerian Prof. Chem. Royal Institution, Corr. Memb. Royal and Imp. Acadd. of Sciences, Paris, Petersburgh, Florence, Copenhagen, Berlin, &c. &c.*

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§. 18. *On Induction* (*continued*). ¶ ix. *Disruptive discharge* (*continued*)—*Peculiarities of positive and negative discharge either as spark or brush—Glow discharge—Dark discharge.*—¶ x. *Convection, or carrying discharge.* ¶ xi. *Relation of a vacuum to electrical phenomena.* §. 19. *Nature of the electrical current.*

¶ ix. *Disruptive discharge* (*continued*).

1480. Let us now direct our attention to the general difference of the positive and negative disruptive discharge, with the object of tracing, as far as possible, the cause of that difference, and whether it depends on the charged conductors principally, or on the interposed dielectric; and as it appears to be great in air and nitrogen (1476.), let us observe the phenomena in air first.

1481. The general case is best understood by a reference to surfaces of considerable size rather than to points, which involve (as a secondary effect) the formation of currents (1562.). My investigation, therefore, was carried on with balls and terminations of different diameters, and the following are some of the principal results.

1482. If two balls of very different dimensions, as for instance one, half an inch, and the other three inches, in diameter, be arranged at the ends of rods so that either can be electrified by a machine and made to discharge by sparks to the other, which is at the same time uninsulated; then, as is well known, far longer sparks are obtained when the small ball is positive and the large ball negative, than when the small ball is negative and the large ball positive. In the former case, the sparks are 10 or 12 inches in length; in the latter an inch or an inch and a half only.

1483. But previous to the description of further experiments, I will mention two words, for which with many others I am indebted to a friend, and which I think it would be expedient to introduce and use. It is important in ordinary inductive action, to distinguish at which charged surface the induction originates and is sustained: i. e. if two or more metallic balls, or other masses of matter, are in inductive relation, to express which are charged originally, and which are brought by them into the

opposite electrical condition. I propose to call those bodies which are originally charged, *inductric* bodies; and those which assume the opposite state, in consequence of the induction, *inducteous* bodies. This distinction is not needful because there is any difference between the sums of the *inductric* and the *inducteous* forces; but principally because, when a ball A is inductric, it not merely brings a ball B, which is opposite to it, into an inducteous state, but also many other surrounding conductors, though some of them may be a considerable distance off, and the consequence is, that the balls do not bear the same precise relation to each other when, first the one, and then the other, is made the inductric ball; though, in each case, the *same ball* be made to assume the *same state.*

1484. Another liberty which I may also occasionally take in language I will explain and limit. It is that of calling a particular spark or brush, *positive* or *negative*, according as it may be considered as *originating* at a positive or a negative surface. We speak of the brush as positive or negative when it shoots out from surfaces previously in those states; and the experiments of Mr. Wheatstone go to prove that it *really begins* at the charged surface, and from thence extends into the air (1437. 1438.) or other dielectric. According to my view, *sparks* also originate or are determined at one particular spot (1370.), namely, that where the tension first rises up to the maximum degree; and when this can be determined, as in the simultaneous use of large and small balls, in which case the discharge begins or is determined by the latter, I would call that discharge which passes *at once*, a positive spark, if it was at the positive surface that the maximum intensity was first obtained, or a negative spark, if that necessary intensity was first obtained at the negative surface.

1485. An apparatus was arranged, as in fig. 15. (Plate III.): A and B, were brass balls of very different diameters attached to metal rods, moving through sockets on insulating pillars, so that the distance between the balls could be varied at pleasure. The large ball A, 2 inches in diameter, was connected with an insulated brass conductor, which could be rendered positive or negative directly from a cylinder machine: the small ball B, 0·25 of an inch in diameter, was connected with a discharging train (292.) and perfectly uninsulated. The brass rods sustaining the balls were 0·2 of an inch in thickness.

1486. When the large ball was *positive* and inductric (1483.), negative sparks occurred until the interval was 0·49 of an inch; then mixed brush and spark between that and 0·51; and from 0·52 and upwards, negative brush alone. When the large ball was made *negative* and inductric, then positive spark alone occurred until the interval was as great as 1·15 inches; spark and brush from that up to 1·55; and to have the positive brush alone, it required an interval of at least 1·65 inches.

1487. The balls A and B were now changed for each other. Then making the small ball B inductric *positively*, the positive sparks alone continued only up to 0·67; spark and brush occurred from 0·68 up to 0·72; and positive brush alone from 0·74 and upwards. Rendering the small ball B inductric and *negative*, negative sparks

alone occurred up to 0·40; then spark and brush at 0·42; whilst from 0·44 and upwards the noisy negative brush alone took place.

1488. We thus find a great difference as the balls are rendered inductric or inducteous; the small ball rendered *positive* inducteously giving a spark nearly twice as long as that produced when it was charged positive inductrically, and a similar difference, though not, under the circumstances, to the same extent, was manifest when it was rendered *negative*.

1489. Another result is, that the small ball rendered positive gives a much longer spark than when it is rendered negative, and that the small ball rendered negative gives a brush more readily than when positive, in relation to the effect of increasing distance.

1490. When the interval was below 0·4 of an inch, so that the small ball should give sparks, whether positive or negative, I could not observe that there was any constant difference, either in their ready occurrence or the number which passed in a given time. But when the interval was such that the small ball when negative gave a brush, then the discharges from it, as separate negative brushes, were far more numerous than the corresponding discharges from it when rendered positive, whether those positive discharges were as sparks or brushes.

1491. It is, therefore, evident that, when a ball is discharging electricity in the form of brushes, the brushes are far more numerous, and each contains or carries off far less electric force when the electricity so discharged is negative, than when it is positive.

1492. In all such experiments as those described, the point of change from spark to brush is very much governed by the working state of the electrical machine and the size of the conductor connected with the discharging ball. If the machine be in strong action and the conductor large, so that much power is accumulated quickly for each discharge, then the interval is greater at which the sparks are replaced by brushes; but the general effect is the same.

1493. These results, though indicative of very striking and peculiar relations of the electric force or forces, do not show the relative degrees of charge which the small ball acquires before discharge occurs, i. e. they do not tell whether it acquires a higher condition in the negative, or in the positive state, immediately preceding that discharge. To illustrate this important point I arranged two places of discharge as represented, fig. 16. A and D, are brass balls 2 inches in diameter, B and C are smaller brass balls 0·25 of an inch in diameter; the forks L and R supporting them were of brass wire 0·2 of an inch in diameter: the space between the large and small ball on the same fork was 5 inches, that the two places of discharge *n* and *o* might be sufficiently removed from each other's influence. The fork L was connected with a projecting cylindrical conductor, which could be rendered positive or negative at pleasure, by an electrical machine, and the fork R was attached to another conductor, but thrown into an uninsulated state by connection with a dis-

charging train. The two intervals or places of discharge *n* and *o* could be varied at pleasure, their extent being measured by the occasional introduction of a diagonal scale. It is evident, that, as the balls A and B connected with the same conductor are always charged at once, and that discharge may take place to either of the balls connected with the discharging train, the intervals of discharge *n* and *o* may be properly compared to each other, as respects the influence of large and small balls when charged positively and negatively in air.

1494. When the intervals *n* and *o* were each made = 0·9 of an inch, and the balls A and B inductric *positively*, the discharge was all at *n* from the small ball of the conductor to the large ball of the discharging train, and mostly by positive brush, though once by a spark. When the balls A and B were made inductric *negatively*, the discharge was still from the same small ball, at *n*, by a constant negative brush.

1495. I diminished the intervals *n* and *o* to 0·6 of an inch. When A and B were inductric *positively*, all the discharge was at *n* as a positive brush: when A and B were inductric *negatively*, still all the discharge was at *n*, as a negative brush.

1496. The facility of discharge at the positive and negative small balls, therefore, did not appear to be very different. If a difference had existed, there were always two small balls, one in each state, that the discharge might happen at that most favourable to the effect. The only difference was, that one was in the inductric, and the other in the inducteous state, but whichever happened for the time to be in that state, whether positive or negative, had the advantage.

1497. To counteract this interfering influence, I made the interval *n* = 0·79 and interval *o* = 0·58 of an inch. Then, when the balls A and B were *inductric positive*, the discharge was about equal at the two intervals. When, on the other hand, the balls A and B were inductric *negative*, there was discharge, still at both, but most at *n*, as if the small ball *negative* could discharge a little easier than the same ball *positive*.

1498. The small balls and terminations used in these and similar experiments may very correctly be compared, in their action, to the same balls and ends when electrified in free air at a much greater distance from conductors, than they were in those cases from each other. In the first place, the discharge, even when as a spark, is, according to my view, determined, and, so to speak, begins at a spot on the surface of the small ball (1374.), occurring when the intensity there has risen up to a certain maximum limiting degree (1370.); this determination of discharge at a particular spot first, being easily traced from the spark into the brush, by increasing the distance, so as, at last, even to render evident the time which is necessary (1436. 1438.). In the next place, the large balls which I have used might be replaced by larger balls at a still greater distance, and so, by successive degrees, may be considered as passing into the sides of the rooms; these being under general circumstances the inducteous bodies, whilst the small ball rendered either positive or negative is the inductric body.

1499. But, as has long been recognised, the small ball is only a blunt end, and, electrically speaking, a point only a small ball; so that when a point or blunt end is

throwing out its brushes into the air, it is acting exactly as the small balls have acted in the experiments already described, and by virtue of the same properties and relations.

1500. It may very properly be said with respect to the experiments, that the large negative ball is as essential to the discharge as the small positive ball, and also that the large negative ball shows as much superiority over the large positive ball (which is inefficient in causing a spark from its opposed small negative ball) as the small positive ball does over the small negative ball; and probably when we understand the real cause of the difference, and refer it rather to the condition of the particles of the dielectric than to the sizes of the conducting balls, we may find much importance in such an observation. But for the present, and whilst engaged in investigating the point, we may admit, what is the fact, that the forces are of higher intensity at the surfaces of the smaller balls than at those of the larger (1372. 1374.); that the former, therefore, determine the discharge, by first rising up to that exalted condition which is necessary for it; and that, whether brought to this condition by induction towards the walls of a room or the large balls I have used, these may fairly be compared one with the other in their influence and actions.

1501. The conclusions I arrive at are: first, that when two equal small conducting surfaces equally placed in air are electrified, one positively and the other negatively, that which is negative can discharge to the air at a tension a little lower than that required for the positive ball: second, that when discharge does take place, much more passes at each time from the positive than from the negative surface (1491.). The last conclusion is very abundantly proved by the optical analysis of the positive and negative brushes already described (1468.), the latter set of discharges being found to recur five or six times oftener than the former*.

1502. If, now, a small ball be made to give brushes or brushy sparks by a powerful machine, we can, in some measure, understand and relate the difference perceived when it is rendered positive or negative. It is known to give when positive a much larger and more powerful spark than when negative, and with greater facility (1482.); in fact, the spark, although it takes away so much more electricity at once, commences at a tension higher only in a small degree, if at all. On the other hand, if rendered negative, though discharge may commence at a lower degree, it continues but for a very short period, very little electricity passing away each time. These circumstances are directly related, for the extent to which the positive spark can reach, and the size and extent of the positive brush, are consequences of the capability which exists of much electricity passing off at one discharge from the positive surface (1468. 1501.).

1503. But to relate these effects only to the form and size of the conductor, would, according to my notion of induction, be a very imperfect mode of viewing the whole

* A very excellent mode of examining the relation of small positive and negative surfaces would be by the use of drops of gum water, solutions, or other liquids. See onwards (1581. 1593.).

question (1523.). I expect that the effects are due altogether to the mode in which the particles of the interposed dielectric polarize, and I have already given some experimental indications of the differences presented by different dielectrics in this respect (1475. 1476.). The modes of polarization, as I shall have occasion hereafter to show, may be very diverse in different dielectrics. With respect to common air, what seems to be the consequence of a superiority in the positive force at the surface of the small ball, may be due to the more exalted condition of the negative polarity of the particles of air, or of the nitrogen in it (the negative part being, perhaps, more compressed, whilst the positive part is more diffuse, or *vice versâ*); for such a condition could determine certain effects at the positive ball which would not take place to the same degree at the negative ball, just as well as if the positive ball had possessed some special and independent power of its own.

1504. That the effects are more likely to be dependent upon the dielectric than the ball, is supported by the character of the two discharges. If a small positive ball be throwing off brushes with ramifications ten inches long, how can the ball affect that part of a ramification which is five inches from it? Yet the portion beyond that place has the same character as that preceding it, and no doubt has that character impressed by the same general principle and law. Looking upon the action of the contiguous particles of a dielectric as fully proved, I see, in such a ramification, a propagation of discharge from particle to particle, each doing for the one next it what was done for it by the preceding particle, and what was done for the first particle by the charged metal against which it was situated.

1505. With respect to the general condition and relations of the positive and negative brushes in dense or rare air, or in other media and gases, if they are produced at different times and places, they are of course independent of each other. But when they are produced from opposed ends or balls at the same time, in the same vessel of gas (1470. 1477.), they are frequently related; and circumstances may be so arranged that they shall be isochronous, occurring in equal numbers in equal times; or shall occur in multiples, i. e. with two or three negatives to one positive; or shall alternate, or be quite irregular. All these variations I have witnessed; and when it is considered that the air in the vessel, and also the glass of the vessel, can take a momentary charge, it is easy to comprehend their general nature and cause.

1506. Similar experiments to those in air (1485. 1493.) were made in different gases, the results of which I will describe as briefly as possible. The apparatus is represented fig. 17. consisting of a bell-glass eleven inches in diameter at the widest part, and ten and a half inches high up to the bottom of the neck. The balls are lettered, as in fig. 16, and are in the same relation to each other; but A and B were on separate sliding wires, which, however, were generally joined by a cross wire, *w*, above, and that connected with the brass conductor, which received its positive or negative charge from the machine. The rods of A and B were graduated at the part moving through the stuffing-box, so that the application of a diagonal scale applied there, told what was

the distance between these balls and those beneath them. As to the position of the balls in the jar, and their relation to each other, C and D were three and a quarter inches apart, their height above the pump plate five inches, and the distance between any of the balls and the glass of the jar one and three quarter inches at least, and generally more. The balls A and D were two inches in diameter, as before (1493.): the balls B and C only 0·15 of an inch in diameter.

Another apparatus was occasionally used in connection with the one just described, being an open discharger (fig. 18.), by which a comparison of the discharge in air and that in gases could be obtained. The balls E and F, each 0·6 of an inch in diameter, were connected with sliding rods and other balls, and were insulated. When used for comparison, the brass conductor was associated at the same time with the balls A and B of figure 17 and ball E of this apparatus (fig. 18.); whilst the balls C, D and F were connected with the discharging train.

1507. I will first tabulate the results as to the *restraining power* of the gases over discharge. The balls A and C (fig. 17.) were thrown out of action by distance, and the effects at B and D, or the interval n in the gas, compared with those at the interval p in the air, between E and F (fig. 18.). The Table sufficiently explains itself. It will be understood, that all discharge was in the air, when the interval there was less than that expressed in the first or third columns of figures; and all the discharge in the gas, when the interval in air was greater than that in the second or fourth column of figures. At intermediate distances the discharge was occasionally at both places, i. e. sometimes in the air, sometimes in the gas.

	Interval p in parts of an inch.			
Constant interval n between B and D = 1 inch.	When the small ball B was inductric and *positive* the discharge was all at p in air before.	at n in the gas after.	When the small ball B was inductric and *negative* the discharge was all at p in air before.	at n in the gas after.
	$p =$	$p =$	$p =$	$p =$
In Air	0·40	0·50	0·28	0·33
In Nitrogen	0·30	0·65	0·31	0·40
In Oxygen	0·33	0·52	0·27	0·30
In Hydrogen	0·20	0·40	0·22	0·24
In Coal gas	0·20	0·90	0·20	0·27
In Carbonic acid	0·64	1·30	0·30	0·45

1508. These results are the same generally, as far as they go, as those of the like nature in the last series (1388.), and confirm the conclusion that different gases restrain discharge in very different proportions. They are probably not so good as the former ones, for the glass jar not being varnished, acted irregularly, sometimes taking a certain degree of charge as a non-conductor, and at other times acting as a conductor in the conveyance and derangement of that charge. Another cause of difference in the ratios is, no doubt, the relative sizes of the discharge balls in air; in the former case they were of very different size, here they were alike.

1509. In future experiments intended to have the character of accuracy, the influence of these circumstances ought to be ascertained, and, above all things, the gases themselves ought to be contained in vessels of metal, and not of glass.

1510. The next set of results are those obtained when the intervals *n* and *o* (fig. 17.) were made equal to each other, and relate to the greater facility of discharge at the small ball, when rendered positive or negative (1493.).

1511. In *air*, with the intervals = 0·4 of an inch, A and B being inductric and positive, discharge was nearly equal at *n* and *o*; when A and B were inductric and negative, the discharge was mostly at *n* by negative brush. When the intervals were = 0·8 of an inch, with A and B inductric positively, all discharge was at *n* by positive brush; with A and B inductric negatively, all the discharge was at *n* by a negative brush. It is doubtful, therefore, from these results, whether the negative ball has any greater facility than the positive.

1512. *Nitrogen.*—Intervals *n* and *o* = 0·4 of an inch: A, B inductric positive, discharge at both intervals, most at *n*, by positive sparks; A, B inductric negative, discharge equal at *n* and *o*. The intervals made = 0·8 of an inch: A, B inductric positive, discharge all at *n* by positive brush; A, B inductric negative, discharge most at *o* by positive brush. In this gas, therefore, though the difference is not decisive, it would seem that the positive small ball caused the most ready discharge.

1513. *Oxygen.*—Intervals *n* and *o* = 0·4 of an inch: A, B inductric positive, discharge nearly equal; inductric negative, discharge mostly at *n* by negative brush. Made the intervals = 0·8 of an inch: A, B inductric positive, discharge at *n* and *o*; inductric negative, discharge all at *o* by negative brush. So here the negative small ball seems to give the most ready discharge.

1514. *Hydrogen.*—Intervals *n* and *o* = 0·4 of an inch: A, B inductric positive, discharge nearly equal; inductric negative, discharge mostly at *o*. Intervals = 0·8 of an inch: A and B inductric positive, discharge mostly at *n*, as positive brush; inductric negative, discharge mostly at *o*, as positive brush. Here the positive discharge seems most facile.

1515. *Coal gas.*—*n* and *o* = 0·4 of an inch: A, B inductric positive, discharge nearly all at *o* by negative spark: A, B inductric negative, discharge nearly all at *n* by negative spark. Intervals = 0·8 of an inch, and A, B inductric positive, discharge mostly at *o* by negative brush: A, B inductric negative, discharge all at *n* by negative brush. Here the negative discharge most facile.

1516. *Carbonic acid gas.*— *n* and *o* = 0·4 of an inch: A, B inductric positive, discharge nearly all at *o*, or negative: A, B inductric negative, discharge nearly all at *n*, or negative. Intervals = 0·8 of an inch: A, B inductric positive, discharge mostly at *o*, or negative: A, B inductric negative, discharge all at *n*, or negative. In this case the negative had a decided advantage in facility of discharge.

1517. Thus, if we may trust this form of experiment, the negative small ball has a decided advantage in facilitating disruptive discharge over the positive small ball in

some gases, as in carbonic acid gas and coal gas (1399.), whilst in others that conclusion seems more doubtful; and in others, again, there seems a probability that the positive small ball may be superior. All these results were obtained at very nearly the same pressure of the atmosphere.

1518. I made some experiments in these gases whilst in the air jar (fig. 17.), as to the change from spark to brush, analogous to those in the open air already described (1486. 1487.). I will give, in a Table, the results as to when brush began to appear mingled with the spark; but the after results were so varied, and the nature of the discharge in different gases so different, that to insert the results obtained without further investigation, would be of little use. At intervals less than those expressed the discharge was always by spark.

	Discharge between balls B and D.		Discharge between balls A and C.	
	Small ball B inductric *pos.*	Small ball B inductric *neg.*	Large ball A inductric *pos.*	Large ball A inductric *neg.*
Air	0·55	0·30	0·40	0·75
Nitrogen......	0·30	0·40	0·52	0·41
Oxygen	0·70	0·30	0·45	0·82
Hydrogen	0·20	0·10		
Coal gas	0·13	0·30	0·30	0·14
Carbonic acid ..	0·82	0·43	1·60	above 1·80; had not space.

1519. It is to be understood that sparks occurred at much higher intervals than these; the table only expresses that distance beneath which all discharge was as spark. Some curious relations of the different gases to discharge are already discernible, but it would be useless to consider them until illustrated by further experiments.

1520. I ought not to omit noticing here, that Professor Belli of Milan has published a very valuable set of experiments on the relative dissipation of positive and negative electricity in the air*; he finds the latter far more ready, in this respect, than the former.

1521. I made some experiments of a similar kind, but with sustained high charges; the results were less striking than those of Signore Belli, and I did not consider them as satisfactory. I may be allowed to mention, in connection with the subject, an interfering effect which embarrassed me for a long time. When I threw positive electricity from a given point into the air, a certain intensity was indicated by an electrometer on the conductor connected with the point, but as the operation continued this intensity rose several degrees; then making the conductor negative with the same point attached to it, and all other things remaining the same, a certain degree of tension was observed in the first instance, which also gradually rose as the operation proceeded. Returning the conductor to the positive state, the tension was at first low, but rose as before; and so also when again made negative.

* Bibliotheque Universelle, 1836, September, p. 152.

1522. This result appeared to indicate that the point which had been giving off one electricity, was, by that, more fitted for a short time to give off the other. But on closer examination I found the whole depended upon the inductive reaction of that air, which being charged by the point, and gradually increasing in quantity before it, as the positive or negative issue was continued, diverted and removed a part of the inductive action of the surrounding wall, and thus apparently affected the powers of the point, whilst really it was the dielectric itself that was causing the change of tension.

1523. The results connected with the different conditions of positive and negative discharge will have a far greater influence on the philosophy of electrical science than we at present imagine, especially if, as I believe, they depend on the peculiarity and degree of polarized condition which the molecules of the dielectrics concerned acquire (1503. 1600.). Thus, for instance, the relation of our atmosphere and the earth within it, to the occurrence of spark or brush, must be especial and not accidental. It would not else consist with other meteorological phenomena, also of course dependent on the special properties of the air, and which being themselves in harmony the most perfect with the functions of animal and vegetable life, are yet restricted in their actions, not by loose regulations, but by laws the most precise.

1524. Even in the passage through air of the voltaic current, we see the peculiarities of positive and negative discharge at the two charcoal points; and if these discharges are made to take place simultaneously to mercury, the distinction is still more remarkable.

1525. It seems very possible that the striking difference recently observed and described by my friend Professor DANIELL*, namely, that when a zinc and a copper ball, the same in size, were placed respectively in copper and zinc spheres, also the same in size, and excited by electrolytes or dielectrics of the same strength and nature, the zinc ball far surpassed the zinc sphere in action, may also be connected with these phenomena; for it is not difficult to conceive how the polarity of the particles shall be affected by the circumstance of the positive surface, namely the zinc, being the larger or the smaller of the two inclosing the electrolyte. It is even possible, that with different electrolytes or dielectrics the ratio may be considerably varied, or in some cases even inverted.

Glow discharge.

1526. That form of disruptive discharge which appears as a *glow* (1359. 1405.), is very peculiar and beautiful: it seems to depend on a quick and almost continuous charging of the air close to, and in contact with, the conductor.

1527. *Diminution of the charging surface* will produce it. Thus, when a rod 0·3

* Philosophical Transactions, 1838, p. 47.

of an inch in diameter, with a rounded termination, was rendered positive in free air, it gave fine brushes from the extremity, but occasionally these disappeared, and a quiet phosphorescent continuous glow took their place, covering the whole of the end of the wire, and extending a very small distance from the metal into the air. With a rod 0·2 of an inch in diameter the glow was more readily produced. With still smaller rods, and also with blunt conical points, it occurred still more readily; and with a fine point I could not obtain the brush in free air, but only this glow. The positive glow and the positive star are, in fact, the same.

1528. *Increase of power in the machine* tends to produce the glow; for rounded terminations which will give only brushes where the machine is in weak action, will readily give the glow when it is in good order.

1529. *Rarefaction of the air* wonderfully favours the glow phenomena. A brass ball, two and a half inches in diameter, being made positively inductric in an air-pump receiver, became covered with glow over an area of two inches in diameter, when the pressure was reduced to 4·4 inches of mercury. By a little adjustment the ball could be covered all over with this light. Using a brass ball 1·25 inches in diameter, and making it inducteously positive by an inductric negative point, the phenomena, at high degrees of rarefaction, were exceedingly beautiful. The glow came over the positive ball, and gradually increased in brightness, until it was at last very luminous; and it also stood up like a low flame, half an inch or more in height. On touching the sides of the glass jar this lambent flame was affected, assumed a ring form, like a crown on the top of the ball, appeared flexible, and revolved with a comparatively slow motion, i. e. about four or five times in a second. This ring-shape and revolution are beautifully connected with the mechanical currents (1576.) taking place within the receiver. These glows in rarefied air are often highly exalted in beauty by a spark discharge at the conductor (1551. *Note*).

1530. To obtain a *negative glow* in air at common pressures is difficult. I did not procure it on the rod 0·3 of an inch in diameter by my machine, nor on much smaller rods; and it is questionable as yet, whether, even on fine points, what is called the negative star is a very reduced and minute, but still intermitting brush, or a glow similar to that obtained on a positive point.

1531. In rarefied air the negative glow can easily be obtained. If the rounded ends of two metal rods, about 0·2 of an inch in diameter, are introduced into a globe or jar (the air within being rarefied), and being opposite to each other, are about four inches apart, the glow can be obtained on both rods, covering not only the ends, but an inch or two of the part behind. On using *balls* in the air-pump jar, and adjusting the distance and exhaustion, the negative ball could be covered with glow, whether it were the inductric or the inducteous surface.

1532. When rods are used it is necessary to be aware that, if placed concentrically in the jar or globe, the light on one rod is often reflected by the sides of the vessel on to the other rod, and makes it apparently luminous, when really it is not so. This

effect may be detected by shifting the eye at the time of observation, or avoided by using blackened rods.

1533. It is curious to observe the relation of glow, brush, and spark to each other, as produced by positive or negative surfaces; thus, beginning with spark discharge, it passes into brush much sooner when the surface at which the discharge commences (1484.) is negative, than it does when positive; but proceeding onwards in the order of change, we find that the positive brush passes into *glow* long before the negative brush does. So that, though each presents the three conditions in the same general order, the series are not precisely the same. It is probable, that, when these points are minutely examined, as they must be shortly, we shall find that each different gas or dielectric presents its own peculiar results, dependent upon the mode in which its particles assume polar electric condition.

1534. The glow occurs in all gases in which I have looked for it. These are air, nitrogen, oxygen, hydrogen, coal gas, carbonic acid, muriatic acid, sulphurous acid and ammonia. I thought also that I obtained it in oil of turpentine, but if so it was very dull and small.

1535. The glow is always accompanied by a wind proceeding either directly out from the glowing part, or directly towards it; the former being the most general case. This takes place even when the glow occurs upon a ball of considerable size: and if matters be so arranged that the ready and regular access of air to a part exhibiting the glow be interfered with or prevented, the glow then disappears.

1536. I have never been able to analyse or separate the glow into visible elementary intermitting discharges (1427. 1433.), nor to obtain the other evidence of intermitting action, namely an audible sound (1431.). The want of success, as respects trials made by ocular means, may depend upon the large size of the glow preventing the separation of the visible images: and, indeed, if it does intermit, it is not likely that all parts intermit at once with a simultaneous regularity.

1537. All the effects tend to show, that *glow* is due to a continuous charge or discharge of air; in the former case being accompanied by a current from, and in the latter by one to, the place of the glow. As the surrounding air comes up to the charged conductor, on attaining that spot at which the tension of the particles is raised to the sufficient degree (1370. 1410.), it becomes charged, and then moves off, by the joint action of the forces to which it is subject; and, at the same time that it makes way for other particles to come and be charged in turn, actually helps to form that current by which they are brought into the necessary position. Thus, through the regularity of the forces, a constant and quiet result is produced; and that result is, the charging of successive portions of air, the production of a current, and of a continuous glow.

1538. I have frequently been able to make the termination of a rod, which, when left to itself, would produce a brush, produce in preference a glow, simply by aiding the formation of a current of air at its extremity; and, on the other hand, it is not at all

difficult to convert the glow into brushes, by affecting the current of air (1574. 1579.) or the inductive action near it.

1539. The transition from glow, on the one hand, to brush and spark, on the other, and, therefore, their connexion, may be established in various ways. Those circumstances which tend to facilitate the charge of the air by the excited conductor, and also those which tend to keep the tension at the same degree notwithstanding the discharge, assist in producing the glow; whereas those which tend to resist the charge of the air or other dielectric, and those which favour the accumulation of electric force prior to discharge, which, sinking by that act, has to be exalted before the tension can again acquire the requisite degree, favour intermitting discharge, and, therefore, the production of brush or spark. Thus, rarefaction of the air, the removal of large conducting surfaces from the neighbourhood of the glowing termination, the presentation of a sharp point towards it, help to sustain or produce the glow: but the condensation of the air, the presentation of the hand or other large surface, the gradual approximation of a discharging ball, tend to convert the glow into brush or even spark. All these circumstances may be traced and reduced, in a manner easily comprehensible, to their relative power of assisting to produce, either a *continuous* discharge to the air, which gives the glow; or an interrupted one, which produces the brush, and, in a more exalted condition, the spark.

1540. The rounded end of a brass rod, 0·3 of an inch in diameter, was covered with a positive glow by the working of an electrical machine: on stopping the machine, so that the charge of the connected conductor should fall, the glow changed for a moment into brushes just before the discharge ceased altogether, illustrating the necessity for a certain high continuous charge, for a certain sized termination. Working the machine so that the intensity should be just low enough to give continual brushes from the end in free air, the approach of a fine point changed these brushes into a glow. Working the machine so that the termination presented a continual glow in free air, the gradual approach of the hand caused the glow to contract at the very end of the wire, then to throw out a luminous point, which, becoming a foot stalk (1426.), finally produced brushes with large ramifications.

1541. Greasing the end of a rounded wire will immediately make it produce brushes instead of glow. A ball having a blunt point which can be made to project more or less beyond its surface, at pleasure, can be made to produce every gradation from glow, through brush, to spark.

1542. It is also very interesting and instructive to trace the transition from spark to glow, through the intermediate condition of stream, between ends in a vessel containing air more or less rarefied; but I fear to be prolix.

1543. All the effects show, that the glow is in its nature exactly the same as the luminous part of a brush or ramification, namely a charging of air; the only difference being, that the glow has a continuous appearance from the constant renewal of

the same action in the same place, whereas the ramification is due to a momentary, independent and intermitting action of the same kind.

Dark Discharge.

1544. I will now notice a very remarkable circumstance in the luminous discharge accompanied by negative glow, which may, perhaps, be correctly traced hereafter into discharges of much higher intensity. Two brass rods, 0·3 of an inch in diameter, entering a glass globe on opposite sides, had their ends brought into contact, and the air about them very much rarefied. A discharge of electricity from the machine was then made through them, and whilst that was continued the ends were separated from each other. At the moment of separation a continuous glow came over the end of the negative rod, the positive termination remaining quite dark. As the distance was increased, a purple stream or haze appeared on the end of the positive rod, and proceeded directly outwards towards the negative rod; elongating as the interval was enlarged, but never joining the negative glow, there being always a short dark space between. This space, of about $\frac{1}{16}$th or $\frac{1}{20}$th of an inch, was apparently invariable in its extent and its position, relative to the negative rod; nor did the negative glow vary. Whether the negative end were inductric or inducteous, the same effect was produced. It was strange to see the positive purple haze diminish or lengthen as the ends were separated, and yet this dark space and the negative glow remain unaltered (fig. 19.).

1545. Two balls were then used in a large air pump receiver, and the air rarefied. The usual transitions in the character of the discharge took place; but whenever the luminous stream, which appears after the spark and the brush have ceased, was itself changed into glow at the balls, the dark space occurred, and that whether the one or the other ball was made inductric, or positive, or negative.

1546. Sometimes when the negative ball was large, the machine in powerful action, and the rarefaction high, the ball would be covered over half its surface with glow, and then, upon a hasty observation, would seem to exhibit no dark space: but this was a deception, arising from the overlapping of the convex termination of the negative glow and the concave termination of the positive stream. More careful observation and experiment have convinced me, that when the negative glow occurs it never visibly touches the luminous part of the positive discharge, but that the dark space is always there.

1547. This singular separation of the positive and negative discharge, as far as concerns their luminous character, under circumstances which one would have thought very favourable to their coalescence, is probably connected with their differences when in the form of brush, and is perhaps even dependent on the same

cause. Further, there is every likelihood that the dark parts which occur in feeble sparks are also connected with these phenomena*. To understand them would be very important, for it is quite clear that in many of the experiments, indeed in all that I have quoted, discharge is taking place across the dark part of the dielectric to an extent quite equal to what occurs in the luminous part. This difference in the result would seem to imply a distinction in the modes by which the two electric forces are brought into equilibrium in the respective parts; and looking upon all the phenomena as giving additional proofs, that it is to the condition of the particles of the dielectric we must refer for the principles of induction and discharge, so it would be of great importance if we could know accurately in what the difference of action in the dark and the luminous parts consisted.

1548. The dark discharge through air (1552.), which in the case mentioned is very evident (1544.), leads to the inquiry, whether the particles of air are generally capable of effecting discharge from one to another without becoming luminous; and the inquiry is important, because it is connected with that degree of tension which is necessary to originate discharge (1368. 1370.). Discharge between *air and conductors* without luminous appearances are very common; and non-luminous discharges by carrying currents of air and other fluids (1562. 1595.) are also common enough: but these are not cases in point, for they are not discharges between insulating particles.

1549. An arrangement was made for discharge between two balls (1485.) fig. 15., but, in place of connecting the inducteous ball directly with the discharging train, it was put in communication with the inside coating of a Leyden jar, and the discharging train with the outside coating. Then working the machine, it was found that whenever sonorous and luminous discharge occurred at the balls A B, the jar became charged; but that when these did not occur, the jar acquired no charge: and such was the case when small rounded terminations were used in place of the balls, and also in whatever manner they were arranged. Under these circumstances, therefore, discharge even between the air and conductors was always luminous.

1550. But in other cases, the phenomena are such as to make it almost certain, that dark discharge can take place across air. If the rounded end of a metal rod, 0·15 of an inch in diameter, be made to give a good negative brush, the approach of a smaller end or a blunt point opposite to it will, at a certain distance, cause a diminution of the brush, and a glow will appear on the positive inducteous wire, accompanied by a current of air passing from it. Now, as the air is being charged both at the positive and negative surfaces, it seems a reasonable conclusion, that the charged portions meet somewhere in the interval, and there discharge to each other, without producing any luminous phenomena. It is possible, however, that the air electrified positively at the glowing end may travel on towards the negative surface, and actually form that atmosphere into which the visible negative brushes dart, in which case

* See Professor Johnson's experiments. Silliman's Journal, xxv. p. 57.

dark discharge need not, of necessity, occur. But I incline to the former opinion, and think, that the diminution in size of the negative brush, as the positive glow comes on to the end of the opposed wire, is in favour of that view.

1551. Using rarefied air as the dielectric, it is very easy to obtain luminous phenomena as brushes, or glow, upon both conducting balls or terminations, whilst the interval is dark, and that, when the action is so momentary that I think we cannot refer to currents as affecting discharge across the dark part. Thus if two balls, about an inch in diameter, and 4 or more inches apart, have the air rarefied about them, and are then interposed in the course of discharge, an interrupted or spark current being produced at the machine*, each termination may be made to show luminous phenomena, whilst more or less of the interval is quite dark. The discharge will pass as suddenly as a retarded spark (295. 334.), i. e. in an interval of time almost inappreciably small, and in such a case, I think it must have passed across the dark part as true disruptive discharge, and not by convection.

1552. Hence I conclude that dark disruptive discharge may occur (1547. 1550.); and also, that, in the luminous brush, the visible ramifications may not show the full extent of the disruptive discharge (1444. 1452.), but that each may have a dark outside, enveloping, as it were, every part through which the discharge extends. It is probable, even, that there are such things as dark discharges analogous in form to the brush and the spark, but not luminous in any part (1445.).

1553. The occurrence of dark discharge in any case shows at how low a tension disruptive discharge may occur (1548.), and indicates that the light of the ultimate brush or spark is in no relation to the intensity required (1368. 1370.). So to speak, the discharge begins in darkness, and the light is a mere consequence of the quantity which, after discharge has commenced, flows to that spot and there finds its most facile passage (1418. 1435.). As an illustration of the growth generally of discharge, I may remark that, in the experiments on the transition in oxygen of the discharge from spark to brush (1518.), every spark was immediately preceded by a short brush.

1554. The phenomena relative to dark discharge in other gases, though differing in certain characters from those in air, confirm the conclusions drawn above. The two rounded terminations (1544.) (fig. 19.), were placed in *muriatic acid gas* (1445. 1463.) at the pressure of 6·5 inches of mercury, and a continuous machine current of electricity sent through the apparatus: bright sparks occurred until the interval was about or above an inch, when they were replaced by squat brushy intermitting glows upon both terminations, with a dark part between. When the current at the machine was in spark, then each spark caused a discharge across the muriatic acid gas, which, with a certain interval, was bright; with a larger interval, was straight across

* By spark current I mean one passing in a series of spark between the conductor of the machine and the apparatus: by a continuous current one that passes through metallic conductors, and in that respect without interruption at the same place.

and flamy, like a very exhausted and sudden, but not a dense sharp spark; and with a still larger interval, produced a feeble brush on the inductric positive end, and a glow on the inducteous negative end, the dark part being between (1544.); and at such times, the spark at the conductor, instead of being sudden and sonorous, was dull and quiet (334.).

1555. On introducing more muriatic acid gas, until the pressure was 29·97 inches, the same terminations gave bright sparks within at small distances; but when they were about an inch or more apart, the discharge was generally with very small brushes and glow, and frequently with no light at all, though electricity had passed through the gas. Whenever the bright spark did pass through the muriatic acid gas at this pressure, it was bright throughout, presenting no dark or dull space.

1556. In *coal gas*, at common pressures, when the distance was about an inch, the discharge was accompanied by short brushes on the ends, and a dark interval of half an inch or more between them, notwithstanding the discharge had the sharp quick sound of a dull spark, and could not have depended in the dark part on *convection*.

1557. This gas presents several curious points in relation to the bright and dark parts of spark discharge. When bright sparks passed between the rod ends 0·3 of an inch in diameter (1544.), very sudden dark parts would occur next to the brightest portions of the spark. Again, with these ends and also with balls (1422.), the bright sparks would be sometimes red, sometimes green, and occasionally green and red in different parts of the same spark. Again, in the experiments described (1518.), at certain intervals a very peculiar pale, dull, yet sudden discharge would pass, which, though apparently weak, was very direct in its course, and accompanied by a sharp snapping noise, as if quick in its occurrence.

1558. *Hydrogen* frequently gave peculiar sparks, one part being bright red, whilst the other was a dull pale gray, or else the whole spark was dull and peculiar.

1559. *Nitrogen* presented a very remarkable discharge, between two balls of the respective diameters of 0·15 and 2 inches (1506. 1518.), the smaller one being rendered negative either directly or inducteously. The peculiar discharge occurred at intervals between 0·42 and 0·68, and even at 1·4 inches when the large ball was inductric positively; it consisted of a little brushy part on the small negative ball, then a dark space, and lastly a dull straight line on the large positive ball (fig. 20.). The position of the dark space was very constant, and is probably in direct relation to the dark space described when negative glow was produced (1544.). When by any circumstance a bright spark was determined, the contrast with the peculiar spark described was very striking; for it always had a faint purple part, but the place of this part was constantly near the positive ball.

1560. Thus dark discharge appears to be decidedly established. But its establishment is accompanied by proofs that it occurs in different degrees and modes in different gases. Hence then another specific action, added to the many (1296. 1398. 1399. 1423. 1454. 1503.) by which the electrical relations of insulating dielectrics are distin-

guished and established, and another argument in favour of that molecular theory of induction, which is at present under examination*.

1561. What I have had to say regarding the disruptive discharge has extended to some length, but I hope will be excused in consequence of the importance of the subject. Before concluding my remarks, I will again intimate in the form of a query, whether we have not reason to consider the tension or retention and after discharge in air or other insulating dielectrics, as the same thing with retardation and discharge in a metal wire, differing only, but almost infinitely, in degree (1334. 1336.). In other words, can we not, by a gradual chain of association, carry up discharge from its occurrence in air, through spermaceti and water, to solutions, and then on to chlorides, oxides and metals, without any essential change in its character; and at the same time, connecting the insensible conduction of air, through muriatic acid gas and the dark discharge, with the better conduction of spermaceti, water, and the all but perfect conduction of the metals, associate the phenomena at both extremes? and may it not be, that the retardation and ignition of a wire are effects exactly correspondent in their nature to the retention of charge and spark in air? If so, here again the two extremes in property amongst dielectrics will be found to be in intimate relation, the whole difference probably depending upon the mode and degree in which their particles polarize under the influence of inductive actions (1338. 1603. 1610.).

¶ x. *Convection; or carrying discharge.*

1562. The last kind of discharge which I have to consider is that effected by the motion of charged particles from place to place. It is apparently very different in its nature to any of the former modes of discharge (1319.), but, as the result is the same, may be of great importance in illustrating, not merely the nature of discharge itself, but also of what we call the electric current. It often, as before observed, in cases of brush and glow (1440. 1535.), joins its effect to that of disruptive discharge, to complete the act of neutralization amongst the electric forces.

1563. The particles which being charged, then travel, may be either of insulating or conducting matter, large or small. The consideration in the first place of a large particle of conducting matter may perhaps help our conceptions.

1564. A copper boiler 3 feet in diameter was insulated and electrified, but so feebly, that dissipation by brushes or disruptive discharge did not occur at its edges or projecting parts in a sensible degree. A brass ball, 2 inches in diameter, suspended by

* I cannot resist referring here by a note to Biot's philosophical view of the nature of the light of the electric discharge, Annales de Chimie, liii. p. 321.

a clean white silk thread, was brought towards it, and it was found that, if the ball was held for a second or two near any part of the charged surface of the boiler, at such distance (two inches more or less) as not to receive any direct charge from it, it became itself charged, although insulated the whole time; and its electricity was the *reverse* of that of the boiler.

1565. This effect was the strongest opposite the edges and projecting parts of the boiler, and weaker opposite the sides, or those extended portions of the surface which, according to Coulomb's results, have the weakest charge. It was very strong opposite a rod projecting a little way from the boiler. It occurred when the copper was charged negatively as well as positively: it was produced also with small balls down to 0·2 of an inch and less in diameter, and also with smaller charged conductors than the copper. It is, indeed, hardly possible in some cases to carry an insulated ball within an inch or two of a charged plane or convex surface without its receiving a charge of the contrary kind to that of the surface.

1566. This effect is one of induction, not of communication. The ball, when related to the positive charged surface by the intervening dielectric, has its opposite sides brought into contrary states, that side towards the boiler being negative and the outer side positive. More inductric action is directed towards it than would have passed across the same place if the ball had not been there, for several reasons; amongst others, because, being a conductor, the resistance of the particles of the dielectric, which otherwise would have been there, is removed (1298.); and also, because the reacting positive surface of the ball being projected further out from the boiler than when there is no introduction of conducting matter, is more free therefore to act through the rest of the dielectric towards surrounding conductors, and so favours the exaltation of that inductric polarity which is directed in its course. It is, as to the exaltation of force upon its outer surface beyond that upon the inductric surface of the boiler, as if the latter were itself protuberant in that direction. Thus it acquires a state like, but higher than, that of the surface of the boiler which causes it; and sufficiently exalted to discharge at its positive surface to the air, or to affect small particles, as it is itself affected by the boiler, and they flying to it, take a charge and pass off; and so the ball, as a whole, is brought into the contrary inducteous state. The consequence is, that, if free to move, its tendency, under the influence of all the forces, to approach the boiler is increased, whilst it at the same time becomes more and more exalted in its condition, both of polarity and charge, until, at a certain distance, discharge takes place, it acquires the same state as the boiler, is repelled, and passing to that conductor most favourably circumstanced to discharge it, there resumes its first indifferent condition.

1567. It seems to me, that the manner in which inductric bodies affect uncharged floating or moveable conductors near them, is very frequently of this nature, and generally so when it ends in a carrying operation (1562. 1602.). The manner in which, whilst the dominant inductric body cannot give off its electricity to the air, the in-

ducteous body *can* effect the discharge of the same kind of force, is curious, and, in the case of elongated or irregularly shaped conductors, such as filaments or particles of dust, the effect will often be very ready, and the consequent attraction very immediate.

1568. The effect described is also probably influential in causing those variations in spark discharge referred to in the last series (1386. 1390.): for if a particle of dust were drawn towards the axis of induction between the balls, it would tend, whilst at some distance from that axis, to commence discharge at itself, in the manner described (1566.), and that commencement might so far facilitate the act (1417. 1420.) as to make the complete discharge, as spark, pass through the particle, though it might not be the shortest course from ball to ball. So also, with equal balls at equal distances, as in the experiments of comparison already described (1493. 1506.), a particle being between one pair of balls would cause discharge there in preference; or even if a particle were between each, difference of size or shape would give one for the time a predominance over the other.

1569. The power of particles of dust to carry off electricity in cases of high tension is well known, and I have already mentioned some instances of the kind in the use of the inductive apparatus (1201.). The general operation is very well shown by large light objects, as the toy called the electrical spider; or, if smaller ones are wanted for philosophical investigation, by the smoke of a glowing green wax taper, which, presenting a successive stream of such particles, makes their course visible.

1570. On using oil of turpentine as the dielectric, the action and course of small conducting carrying particles in it can be well observed. A few short pieces of thread will supply the place of carriers, and their progressive action is exceedingly interesting.

1571. A very striking effect was produced on oil of turpentine, which, whether it was due to the carrying power of the particles in it, or to any other action of them, is perhaps as yet doubtful. A portion of that fluid in a glass vessel had a large uninsulated silver dish at the bottom, and an electrified metal rod with a round termination dipping into it at the top. The insulation was very good, and the attraction and other phenomena striking. The rod end, with a drop of gum water attached to it, was then electrified in the fluid; the gum water soon spun off in fine threads, and was quickly dissipated through the oil of turpentine. By the time that four drops had in this way been commingled with a pint of the dielectric, the latter had lost by far the greatest portion of its insulating power; no sparks could be obtained in the fluid; and all the phenomena dependent upon insulation had sunk to a low degree. The fluid was very slightly turbid. Upon being filtered through paper only, it resumed its first clearness, and now insulated as well as before. The water, therefore, was merely diffused through the oil of turpentine, not combined with or dissolved in it: but whether the minute particles acted as carriers, or whether they were not rather gathered together in the line of highest inductive tension (1350.), and there,

being drawn into elongated forms by the electric forces, combined their effects to produce a band of matter having considerable conducting power, as compared with the oil of turpentine elsewhere, is as yet questionable.

1572. The analogy between the action of solid conducting carrying particles and that of the charged particles of fluid insulating substances, acting as dielectrics, is very evident and simple; but in the latter case the result is, necessarily, currents in the mobile media. Particles are brought by inductric action into a polar state; and the latter, after rising to a certain tension (1370.), is followed by the communication of a part of the force originally on the conductor; the particles consequently become charged, and then, under the joint influence of the repellent and attractive forces, are urged towards a discharging place, or to that spot where these inductric forces are most easily compensated by the contrary inducteous forces.

1573. Why a point should be so exceedingly favourable to the production of currents in a fluid insulating dielectric, as air, is very evident. It is at the extremity of the point that the intensity necessary to charge the air is first acquired (1374.); it is from thence that the charged particle recedes; and the mechanical force which it impresses on the air to form a current, is in every way favoured by the shape and position of the rod, of which the point forms the termination. At the same time, the point, having become the origin of an active mechanical force, does, by the very act of causing that force, namely, by discharge, prevent any other part of the rod from acquiring the same necessary condition, and so preserves and sustains its own predominance.

1574. The very varied and beautiful phenomena produced by sheltering or enclosing the point, illustrate the production of the current exceedingly well, and justify the same conclusions; it being remembered that in such cases the effect upon the discharge is of two kinds. For the current may be interfered with by stopping the access of fresh uncharged air, or retarding the removal of that which has been charged, as when a point is electrified in a tube of insulating matter closed at one extremity; or the *electric condition* of the point itself may be altered by the relation of other parts in its neighbourhood, also rendered electric, as when the point is in a metal tube, by the metal itself, or when it is in the glass tube, by a similar action of the charged parts of the glass, or even by the surrounding air which has been charged, and which cannot escape.

1575. Whenever it is intended to observe such inductive phenomena in a fluid dielectric as have a direct relation to, and dependence upon, the fluidity of the medium, such, for instance, as a discharge from points, or attractions and repulsions, &c., then the mass of the fluid should be great, and in such proportion to the distance between the inductric and inducteous surfaces as to include all the *lines of inductive force* (1369.) between them; otherwise, the effects of currents, attraction, &c., which are the resultants of all these forces, cannot be obtained. The phenomena which occur in the open air, or in the middle of a globe filled with oil of turpentine, will not take place

in the same media if confined in tubes of glass, shell-lac, sulphur, or other such substances, though they be excellent insulating dielectrics; nor can they be expected; for in such cases, the polar forces, instead of being all dispersed amongst fluid particles, which tend to move under their influence, are now associated in many parts with particles that, notwithstanding their tendency to motion, are constrained to remain quiescent.

1576. The varied circumstances under which, with conductors differently formed and constituted, currents can occur, all illustrate the same simplicity of production. A *ball*, if the intensity be raised sufficiently on its surface, and that intensity be greatest on a part consistent with the production of a current of air up to and off from it, will produce the effect like a point (1537.); such is the case whenever the glow occurs upon a ball, the current being essential to that phenomenon. If as large a sphere as can well be employed with the production of glow be used, the glow will appear at the place where the current leaves the ball, and that will be the part directly opposite to the connection of the ball and rod which supports it; but by increasing the tension elsewhere, so as to raise it above the tension upon that spot, which can easily be effected inductively, then the place of the glow and the direction of the current will also change, and pass to that spot which for the time is most favourable for their production (1591.).

1577. For instance, approaching the hand towards the ball will tend to cause brush (1539.), but by increasing the supply of electricity the condition of glow may be preserved; then on moving the hand about from side to side the position of the glow will very evidently move with it.

1578. A point brought towards a glowing ball would at twelve or fourteen inches distance make the glow break into brush, but when still nearer glow was reproduced, probably dependent upon the discharge of wind or air passing from the point to the ball, and this glow was very obedient to the motion of the point, following it in every direction.

1579. Even a current of wind could affect the place of the glow; for a varnished glass tube being directed sideways towards the ball, air was sometimes blown through it at the ball, and sometimes not. In the former case, the place of the glow was changed a little, as if it were blown away by the current, and this is just the result which might have been anticipated. All these effects illustrate beautifully the general causes and relations, both of the glow and the current of air accompanying it (1574).

1580. Flame facilitates the production of a current in the dielectric surrounding it. Thus, if a ball which would not occasion a current in the air have a flame, whether large or small, formed on its surface, the current is produced with the greatest ease; but not the least difficulty can occur in comprehending the effective action of the flame in this case, if its relation, as part of the surrounding dielectric, to the electrified ball, be but for a moment considered (1375. 1380.).

1581. Conducting fluid terminations, instead of rigid points, illustrate in a very beautiful manner the formation of the currents, with their effects and influence in exalting the conditions under which they were commenced. Let the rounded end of a brass rod, 0·3 of an inch or thereabouts in diameter, point downwards in free air; let it be amalgamated, and have a drop of mercury suspended from it; and then let it be powerfully electrized. The mercury will present the phenomenon of *glow*; a current of air will rush along the rod, and set off from the mercury directly downwards; and the form of the metallic drop will be slightly affected, the convexity at a small part near the middle and lower part becoming greater, whilst it diminishes all round at places a little removed from this spot. The change is from the form of *a* (fig. 21.) to that of *b*, and is due almost, if not entirely, to the mechanical force of the current of air sweeping over its surface.

1582. As a comparative observation, let it be noticed, that a ball gradually brought towards it converts the glow into brushes, and ultimately sparks pass from the most projecting part of the mercury. A point does the same, but at much smaller distances.

1583. Take next a drop of strong solution of muriate of lime; being electrified, a part will probably be dissipated, but a considerable portion, if the electricity be not too powerful, will remain, forming a conical drop (fig. 22.), accompanied by a strong current. If glow be produced, the drop will be smooth on the surface. If a short low brush is formed, a minute tremulous motion of the liquid will be visible; but both effects coincide with the principal one to be observed, namely, the regular and continuous charge of air, the formation of a wind or current, and the form given by that current to the fluid drop. If a discharge ball be gradually brought toward the cone, sparks will at last pass, and these will be from the apex of the cone to the approached ball, indicating a considerable degree of conducting power in this fluid.

1584. With a drop of water, the effects were of the same kind, and were best obtained when a portion of gum water or of syrup hung from a ball (fig. 23.). When the machine was worked slowly, a fine large quiet conical drop, with concave lateral outline, and a small rounded end, was produced, on which the glow appeared, whilst a steady wind issued, in a direction from the point of the cone, of sufficient force to depress the surface of uninsulated water held opposite to the termination. When the machine was worked more rapidly some of the water was driven off; the smaller pointed portion left was roughish on the surface, and the sound of successive brush discharges was heard. With still more electricity, more water was dispersed; that which remained was elongated and contracted, with an alternating motion; a stronger brush discharge was heard, and the vibrations of the water and the successive discharges of the individual brushes were simultaneous. When water from beneath was brought towards the drop, it did not indicate the same regular strong contracted current of air as before; and when the distance was such that sparks passed,

the water beneath was *attracted* rather than driven away, and the current of air *ceased.*

1585. When the discharging ball was brought near the drop in its first quiet glowing state (1582.), it converted that glow into brushes, and caused the vibrating motion of the drop. When still nearer, sparks passed, but they were always from the metal of the rod, over the surface of the water, to the point, and then across the air to the ball. This is a natural consequence of the deficient conducting power of the fluid (1584. 1585.).

1586. Why the drop vibrated, changing its form between the periods of discharging brushes, so as to be more or less acute at particular instants, to be most acute when the brush issued forth, and to be isochronous in its action, and how the quiet glowing liquid drop, on assuming the conical form, facilitated, as it were, the first action, are points, as to theory, so evident, that I will not stop to speak of them. The principal thing to observe at present is, the formation of the carrying current of air, and the manner in which it exhibits its existence and influence by giving form to the drop.

1587. That the drop, when of water, or a better conductor than water, is formed into a cone principally by the current of air, is shown amongst other ways (1594.) thus. A sharp point being held opposite the conical drop, the latter soon lost its pointed form; was retracted and became round; the current of air from it ceased, and was replaced by one from the point beneath, which, if the latter were held near enough to the drop, actually blew it aside, and rendered it concave in form.

1588. It is hardly necessary to say what happened with still worse conductors than water, as oil, or oil of turpentine; the fluid itself was then spun out into threads and carried off, not only because the air rushing over its surface, helped to sweep it away, but also because its insulating particles assumed the same charged state as the particles of air, and, not being able to discharge to them in a greater degree than the air particles themselves could do, were carried off by the same causes which urged these in their course. A similar effect with melted sealing-wax on a metal point forms an old and well-known experiment.

1589. A drop of gum water in the exhausted receiver of the air-pump was not sensibly affected in its form when electrified. When air was let in, it began to show change of shape when the pressure was ten inches of mercury. At the pressure of fourteen or fifteen inches the change was more sensible, and as the air increased in density the effects increased, until they were the same as those in the open atmosphere. The diminished effect in the rare air I refer to the relative diminished energy of its current; that diminution depending, in the first place, on the lower electric condition of the electrified ball in the rarefied medium, and in the next, on the attenuated condition of the dielectric, the cohesive force of water in relation to rarefied air being something like that of mercury to dense air (1581.), whilst that of water in dense air may be compared to that of mercury in oil of turpentine (1597.).

1590. When a ball is covered with a thick conducting fluid, as treacle or syrup, it is easy by inductive action to determine the wind from almost any part of it (1577.); the experiment, which before was of rather difficult performance, being rendered facile in consequence of the fluid enabling that part, which at first was feeble in its action, to rise into an exalted condition by assuming a pointed form.

1591. To produce the current, the electric intensity must rise and continue at *one spot*, namely, at the origin of the current, higher than elsewhere, and then, air having a uniform and ready access, the current is produced. If no current be allowed (1574.), then discharge may take place by brush or spark. But whether it be by brush or spark, or wind, it seems very probable that the initial intensity or tension at which a particle of a given gaseous dielectric charges, or commences discharge, is, under the conditions before expressed, always the same (1410.).

1592. It is not supposed that all the air which enters into motion is electrified; on the contrary, much that is not charged is carried on into the stream. The part which is really charged may be but a small proportion of that which is ultimately set in motion (1442.).

1593. When a drop of gum water (1584.) is made *negative*, it presents a larger cone than when made positive; less of the fluid is thrown off, and yet, when a ball is approached, sparks can hardly be obtained, so pointed is the cone, and so free the discharge. A point held opposite to it did not cause the retraction of the cone to such an extent as when it was positive. All the effects are so different from those presented by the positive cone, that I have no doubt such drops would present a very instructive method of investigating the difference of positive and negative discharge in air and other dielectrics (1480. 1501.).

1594. That I may not be misunderstood (1587.), I must observe here that I do not consider the cones produced as the result *only* of the current of air or other insulating dielectric over their surface. When the drop is of badly conducting matter, a part of the effect is due to the electrified state of the particles, and this part constitutes almost the whole when the matter is sealing-wax, oil of turpentine, and similar insulating bodies (1588.). But even when the drop is of good conducting matter, as water, solutions, or mercury, though the effect above spoken of will then be insensible 1607.), still it is not the mere current of air or other dielectric which produces all the change of form; for a part is due to those attractive forces by which the charged drop, if free to move, would travel along the line of strongest induction, and not being free to move, has its form elongated until the *sum* of the different forces tending to produce this form is balanced by the cohesive attraction of the fluid. The effect of the attractive forces are well shown when treacle, gum water, or syrup is used; for the long threads which spin out, at the same time that they form the axes of the currents of air, which may still be considered as determined at their points, are like flexible conductors, and show by their directions in what way the attractive forces draw them.

1595. When the phenomena of currents are observed in dense insulating dielectrics, they present us with extraordinary degrees of mechanical force. Thus, if a pint of well rectified and filtered (1571.) oil of turpentine be put into a glass vessel, and two wires be dipped into it in different places, one leading to the electrical machine, and the other to the discharging train, on working the machine the fluid will be thrown into violent motion throughout its whole mass, whilst at the same time it will rise two, three, or four inches up the machine wire, and dart off in jets from it into the air.

1596. If very clean uninsulated mercury be at the bottom of the fluid, and the wire from the machine be terminated either by a ball or a point, and also pass through a glass tube extending both above and below the surface of the oil of turpentine, the currents can be better observed, and will be seen to rush down the wire, proceeding directly from it towards the mercury, and there, diverging in all directions, will ripple its surface strongly, and mounting up at the sides of the vessel, will return to re-enter upon their course.

1597. A drop of mercury being suspended from an amalgamated brass ball, preserved its form almost unchanged in air (1581.); but when immersed in the oil of turpentine it became very pointed, and even particles of the metal could be spun out and carried off by the currents of the dielectric. The form of the liquid metal was just like that of the syrup in air (1584.), the point of the cone being quite as fine, though not so long. By bringing a sharp uninsulated point towards it, it could also be effected in the same manner as the syrup drop in air (1587.), though not so readily, because of the density and limited quantity of the dielectric.

1598. If the mercury at the bottom of the fluid be connected with the electrical machine, whilst a rod is held in the hand terminating in a ball three quarters of an inch, less or more, in diameter, and the ball be dipped into the electrified fluid, very striking appearances ensue. When the ball is raised again so as to be at a level nearly out of the fluid, large portions of the latter will seem to cling to it (fig. 24.). If it be raised higher, a column of the oil of turpentine will still connect it with that in the basin below (fig. 25.). If the machine be excited into more powerful action, this will become more bulky, and may then also be raised higher, assuming the form fig. 26; and all the time that these effects continue, currents and counter-currents, sometimes running very close together, may be observed in the raised column of fluid.

1599. It is very difficult to decide by sight the direction of the currents in such experiments as these. If particles of silk are introduced they cling about the conductors; but using drops of water and mercury the course of the fluid dielectric seems well indicated. Thus, if a drop of water be placed at the end of a rod (1571.) over the uninsulated mercury, it is soon swept away in particles streaming downwards towards the mercury. If another drop be placed on the mercury beneath the end of the rod, it is quickly dispersed in all directions in the form of streaming particles, the attractive forces drawing it into elongated portions, and the currents carrying them away

If a drop of mercury be hung from a ball used to raise a column of the fluid (1598.), then the shape of the drop seems to show currents travelling in the fluid in the direction indicated by the arrows (fig. 27.).

1600. A very remarkable effect is produced on these phenomena, connected with positive and negative charge and discharge, namely, that a ball charged positively raises a much higher and larger column of the oil of turpentine than when charged negatively. There can be no doubt that this is connected with the difference of positive and negative action already spoken of (1480. 1525.), and tends much to strengthen the idea that such difference is referable to the particles of the dielectric rather than to the charged conductors, and is dependent upon the mode in which these particles polarize (1503. 1523.).

1601. Whenever currents travel in insulating dielectrics they really effect discharge; and it is important to observe, though a very natural result, that it is indifferent which way the current or particles travel, as with reversed direction their state is reversed. The change is easily made, either in air or oil of turpentine, between two opposed and related rods, for an insulated ball being placed in connexion with either rod and brought near its extremity, will cause the current to set towards it from the opposite end.

1602. The two currents often occur at once, as when both terminations present brushes, and frequently when they exhibit the glow (1531.). In such cases, the charged particles, or many of them, meet and mutually discharge each other (1548. 1612.). If a smoking wax taper be held at the end of an insulating rod towards a charged prime conductor, it will very often happen that two currents will form, and be rendered visible by its vapour, one passing as a fine filament of smoky particles directly to the charged conductor, and the other passing as directly from the same taper wick outwards, and from the conductor; the principles of inductric action and charge, which were referred to in considering the relation of a carrier ball and a conductor (1566.), being here also called into play.

1603. The general analogy and, I think I may say, identity of action found to exist as to insulation and conduction (1338. 1561.) when bodies, the best and the worst in the classes of insulators or conductors, were compared, led me to believe that the phenomena of *convection* in badly conducting media were not without their parallel amongst the best conductors, such even as the metals. Upon consideration, the cones produced by Davy* in fluid metals, as mercury and tin, seemed to be cases in point, and probably also the elongation of the metallic medium through which a current of electricity was passing, described by Ampere†; for it is not difficult to conceive,

* Philosophical Transactions, 1823, p. 155.

† Bibliotheque Universelle, xxi. 47.

that the diminution of convective effect, consequent upon the high conducting power of the metallic media used in these experiments, might be fully compensated for by the enormous quantity of electricity passing. In fact, it is impossible not to expect *some* effect, whether sensible or not, of the kind in question, when such a current is passing through a fluid offering a sensible resistance to the passage of the electricity, and, thereby, giving proof of a certain degree of insulating power (1328.).

1604. I endeavoured to connect the convective currents in air, oil of turpentine, &c. and those in metals, by intermediate cases, but found this not easy to do. On taking bodies, for instance, which, like water, acids, solutions, fused salts or chlorides, &c., have intermediate conducting powers, the minute quantity of electricity which the common machine can supply (371. 861.) is exhausted instantly, so that the cause of the phenomenon is kept either very low in intensity, or the instant of time during which the effect lasts is so small, that one cannot hope to observe the result sought for. If a voltaic battery be used, these bodies are all electrolytes, and the evolution of gas, or the production of other changes, interferes and prevents observation of the effect required.

1605. There are, nevertheless, some experiments which illustrate the connection. Two platina wires, forming the electrodes of a powerful voltaic battery, were placed side by side, near each other, in distilled water, hermetically sealed up in a strong glass tube, some minute filaments being present in the water. When, from the evolution of gas and the consequent increased pressure, the bubbles formed on the electrodes were so small as to produce but feebly ascending currents, then it could be observed that the filaments present were attracted and repelled between the two wires, as they would have been between two oppositely charged surfaces in air or oil of turpentine, moving so quickly as to displace and disturb the bubbles and the currents which these tended to form. Now I think it cannot be doubted that under similar circumstances, and with an abundant supply of electricity, of sufficient tension also, convective currents might have been formed; the attractions and repulsions of the filaments were, in fact, the elements of such currents (1572.), and therefore water, though almost infinitely above air or oil of turpentine as a conductor, is a medium in which similar currents can take place.

1606. I had an apparatus made (fig. 28.) in which *a* is a plate of shell-lac, *b* a fine platina wire passing through it, and having only the section of the wire exposed above; *c* a ring of bibulous paper resting on the shell-lac, and *d* distilled water retained by the paper in its place, and just sufficient in quantity to cover the end of the wire *b*; another wire, *e*, touched a piece of tin foil lying in the water, and was also connected with a discharging train; in this way it was easy, by rendering *b* either positive or negative, to send a current of electricity by its extremity into the fluid, and so away by the wire *e*.

1607. On connecting *b* with the conductor of a powerful electrical machine, not the least disturbance of the level of the fluid over the end of the wire during the

working of the machine could be observed; but at the same time there was not the smallest indication of electrical charge about the conductor of the machine, so complete was the discharge. I conclude that the quantity of electricity passed in a *given time* had been too small, when compared with the conducting power of the fluid, to produce the desired effect.

1608. I then charged a large Leyden battery (291.), and discharged it through the wire *b*, interposing, however, a wet thread, two feet long, to prevent a spark in the water, and to reduce what would else have been a sudden violent discharge into one of more moderate character, enduring for a sensible length of time (334.). I now did obtain a very brief elevation of the water over the end of the wire; and though a few minute bubbles of gas were at the same time formed there, so as to prevent me from asserting that the effect was unequivocally the same as that obtained by Davy in the metals, yet, according to my best judgement, it was partly, and I believe principally, of that nature.

1609. I employed a voltaic battery of 100 pair of four-inch plates for experiments of a similar nature with electrolytes. In these cases the shell-lac was cupped, and the wire *b* 0·2 of an inch in diameter. Sometimes I used a positive amalgamated zinc wire in contact with dilute sulphuric acid; at others, a negative copper wire in a solution of sulphate of copper; but, because of the evolution of gas, the precipitation of copper, &c., I was not able to obtain decided results. It is but right to mention, that when I made use of mercury, endeavouring to repeat Davy's experiment, the battery of 100 pair was not sufficient to produce the elevations*.

1610. The latter experiments (1609.) may therefore be considered as failing to give the hoped-for proof, but I have much confidence in the former (1605. 1608.), and in the considerations (1603.) connected with them. If I have rightly viewed them, and we may be allowed to relate the currents at points and surfaces in such extremely different bodies as air and the metals, and admit that they are effects of the *same* kind, differing only in degree and in proportion to the insulating or conducting power of the dielectric used, what great additional argument we obtain in favour of that theory, which in the phenomena of insulation and conduction also, as in these, would link *the same* apparently dissimilar substances together (1336. 1561.); and how completely the general view, which refers all the phenomena to the direct action of the molecules of matter, seems to embrace the various isolated phenomena as they successively come under consideration!

1611. The connection of this convective or carrying effect, which depends upon a certain degree of insulation, with conduction; i. e. the occurrence of both in so many

* In the experiments at the Royal Institution, Sir H. Davy used, I think, 500 or 600 pairs of plates. Those at the London Institution were made with the apparatus of Mr. Pepys, (consisting of an enormous single pair of plates), described in the Philosophical Transactions for 1823, p. 187.

of the substances referred to, as, for instance, the metals, water, air, &c., would lead to many very curious theoretical generalizations, which I must not indulge in here. One point, however, I shall venture to refer to. Conduction appears to be essentially an action of contiguous particles, and the considerations just stated, together with others formerly expressed (1326. 1336, &c.), lead to the conclusion, that all bodies conduct, and by the same process, air as well as metals; the only difference being in the necessary degree of force or tension between the particles which must exist before the act of conduction or transfer from one particle to another can take place.

1612. The question then arises, what is this limiting condition which separates, as it were, conduction and insulation from each other? Does it consist in a difference between the two contiguous particles, or the contiguous poles of these particles in the nature and amount of positive and negative force, no communication or discharge occurring unless that difference rises up to a certain degree, variable for different bodies, but always the same for the same body? Or is it true that, however small the difference between two such particles, if *time* be allowed, equalization of force will take place, even with the particles of such bodies as air, sulphur or lac? In the first case, insulating power in any particular body would be proportionate to the degree of the assumed necessary difference of force; in the second, to the *time* required to equalize equal degrees of difference in different bodies. With regard to airs, one is almost led to expect a permanent difference of force, but in all other bodies, time seems to be quite sufficient to ensure, ultimately, complete conduction. The difference in the modes by which insulation may be sustained, or conduction effected, is not a mere fanciful point, but one of great importance, as being essentially connected with the molecular theory of induction, and the manner in which the particles of bodies assume and retain their polarized state.

¶ xi. *Relation of a vacuum to electrical phenomena.*

1613. It would seem strange if a theory which refers all the phenomena of insulation and conduction, i. e. all electrical phenomena, to the action of contiguous particles, were to omit to notice the assumed possible case of a *vacuum*. Admitting that a vacuum can be produced, it would be a very curious matter indeed to know what its relation to electrical phenomena would be; and as shell-lac and metal are directly opposed to each other, whether a vacuum would be opposed to them both, and allow neither of induction or conduction across it. Mr. Morgan* has said that a vacuum does not conduct. Sir H. Davy concluded from his investigations, that as perfect a vacuum as could be made† did conduct, but does not consider the prepared spaces which he used as absolute vacua. In such experiments I think I have observed the

* Philosophical Transactions, 1785, p. 272. † Ibid. 1822, p. 64.

luminous discharge to be principally on the inner surface of the glass; and it does not appear at all unlikely, that, if the vacuum refused to conduct, still the surface of glass next it might carry on that action.

1614. At one time, when I thought inductive force was exerted in right lines, I hoped to illustrate this important question by making experiments on induction with metallic mirrors (used only as conducting vessels) exposed towards a very clear sky at night time, and of such concavity that nothing but the firmament could be visible from the lowest part of the concave *n*, fig. 29. Such mirrors, when electrified, as by connection with a Leyden jar, and examined by a carrier ball, readily gave electricity at the lowest part of their concavity if in a room; but I was in hopes of finding that, circumstanced as before stated, they would give little or none at the same spot, if the atmosphere above really terminated in a vacuum. I was disappointed in the conclusion, for I obtained as much electricity there as before; but on discovering the action of induction in curved lines (1231.), found a full and satisfactory explanation of the result.

1615. My theory, as far as I have ventured it, does not pretend to decide upon the consequences of a vacuum. It is not at present limited sufficiently, or rendered precise enough, either by experiments relating to spaces void of matter, or those of other kinds, to indicate what would happen in the vacuum case. I have only as yet endeavoured to establish, what all the facts seem to prove, that when electrical phenomena, as those of induction, conduction, insulation and discharge occur, they depend on, and are produced by the action of *contiguous* particles of matter, the next existing particle being considered as the contiguous one; and I have further assumed, that these particles are polarized; that each exhibits the two forces, or the force in two directions (1295. 1298.); and that they act at a distance only by acting on the *contiguous* and intermediate particles.

1616. But assuming that a perfect vacuum were to intervene in the course of the lines of inductive action (1304.), it does not follow from this theory, that the particles on opposite sides of such a vacuum could not act on each other. Suppose it possible for a positively electrified particle to be in the centre of a vacuum an inch in diameter, nothing in my present views forbids that the particle should act at the distance of half an inch on all the particles forming the inner superficies of the bounding sphere, and with a force consistent with the well-known law of the squares of the distance. But suppose the sphere of an inch were full of insulating matter, the electrified particle would not then, according to my notion, act directly on the distant particles, but on those in immediate association with it, employing *all* its power in polarizing them; producing in them negative force equal in amount to its own positive force and directed towards the latter, and positive force of equal amount directed outwards and acting in the same manner upon the layer of particles next in succession. So that ultimately, those particles in the surface of a sphere of half an inch radius, which were acted on *directly* when that sphere was a vacuum, will now be acted on *indirectly* as respects

the central particle or source of action, i. e. they will be polarized in the same way, and with the same amount of force.

§ 19. *Nature of the electric current.*

1617. The word *current* is so expressive in common language, that when applied in the consideration of electrical phenomena we can hardly divest it sufficiently of its meaning, or prevent our minds from being prejudiced by it (283. 511.). I shall use it in its common electrical sense, namely, to express generally a certain condition and relation of electrical forces supposed to be in progression.

1618. A current is produced both by excitement and discharge; and whatsoever the variation of the two general causes may be, the effect remains the same. Thus excitement may occur in many ways, as by friction, chemical action, influence of heat, change of condition, induction, &c.; and discharge has the forms of conduction, electrolyzation, disruptive discharge, and convection; yet the current connected with these actions, when it occurs, appears in all cases to be the same. This constancy in the character of the current, notwithstanding the particular and great variations which may be made in the mode of its occurrence, is exceedingly striking and important; and its investigation and development promise to supply the most open and advantageous road to a true and intimate understanding of the nature of electrical forces.

1619. As yet the phenomena of the current have presented nothing in opposition to the view I have taken of the nature of induction as an action of contiguous particles. I have endeavoured to divest myself of prejudices and to look for contradictions, but I have not perceived any in conductive, electrolytic, convective, or disruptive discharge.

1620. Looking at the current as a *cause*, it exerts very extraordinary and diverse powers, not only in its course and on the bodies in which it exists, but collaterally, as in inductive or magnetic phenomena.

1621. *Electrolytic action.*—One of its direct actions is the exertion of pure chemical force, this being a result which has now been examined to a considerable extent. The effect is found to be *constant* and *definite* for the quantity of electric force discharged (783, &c.); and beyond that, the *intensity* required is in relation to the intensity of the affinity or forces to be overcome (904. 906. 911.). The current and its consequences are here proportionate; the one may be employed to represent the other; no part of the effect of either is lost or gained; so that the case is a strict one, and yet it is the very case which most strikingly illustrates the doctrine that induction is an action of contiguous particles (1164. 1343.).

1622. The process of electrolytic discharge appears to me to be in close analogy, and perhaps in its nature identical with another process of discharge, which at first

seems very different from it, I mean *convection*. In the latter case the particles may travel for yards across a chamber; they may produce strong winds in the air, so as to move machinery; and in fluids, as oil of turpentine, may even shake the hand, and carry heavy metallic bodies about*; and yet I do not see that the force, either in kind or action, is at all different to that by which a particle of hydrogen leaves one particle of oxygen to go to another, or by which a particle of oxygen travels in the contrary direction.

1623. Travelling particles of the air can effect chemical changes just as well as the contact of a fixed platina electrode, or that of a combining electrode, or the ions of a decomposing electrolyte (453. 471.); and in the experiment formerly described, where eight places of decomposition were rendered active by one current (469.), and where charged particles of air in motion were the only electrical means of connecting these parts of the current, it seems to me that the action of the particles of the electrolyte and of the air were essentially the same. A particle of air was rendered positive; it travelled in a certain determinate direction, and coming to an electrolyte, communicated its powers; an equal amount of positive force was accordingly acquired by another particle (the hydrogen), and the latter, so charged, travelled as the former did, and in the same direction, until it came to another particle, and transferred its power and motion, making that other particle active. Now, though the particle of air travelled over a visible and occasionally a large space, whilst the particle of the electrolyte moved over an exceedingly small one; though the air particle might be oxygen, nitrogen, or hydrogen, receiving its charge from force of high intensity, whilst the electrolytic particle of hydrogen had a natural aptness to receive the positive condition with extreme facility; though the air particle might be charged with very little electricity at a very high intensity by one process, whilst the hydrogen particle might be charged with much electricity at a very low intensity by another process; these are not differences of kind, as relates to the final discharging action of these particles, but only of degree; not essential differences which make things unlike, but such differences as give to things, similar in their nature, that great variety which fits them for their office in the system of the universe.

1624. So when a particle of air, or of dust in it, electrified at a negative point, moves on through the influence of the inductive forces (1572.) to the next positive surface, and after discharge passes away, it seems to me to represent exactly that particle of oxygen which, having been rendered negative in the electrolyte, is urged by the same disposition of inductive forces, and going to the positive platina electrode, is there discharged, and then passes away, as the air or dust did before it.

* If a metallic vessel three or four inches deep, containing oil of turpentine, be insulated and electrified, and a rod with a ball (an inch or more in diameter) at the end have the ball immersed in the fluid whilst the end is held in the hand, the mechanical force generated when the ball is moved to and from the sides of the vessel will soon be evident to the experimenter.

1625. *Heat* is another direct effect of the *current* upon substances in which it occurs, and it becomes a very important question, as to the relation of the electric and heating forces, whether the latter is always definite in amount*. There are many cases, even amongst bodies which conduct without change, which stand out at present from the assumption that it is†; but there are also many which indicate that, when proper limitations are applied, the heat produced is definite. Harris has shown this for a given length of current in a metallic wire, using common electricity‡; and De la Rive has proved the same point for voltaic electricity by his beautiful application of Breguet's thermometer§.

1626. When the production of heat is observed in electrolytes under decomposition, the results are still more complicated. But important steps have been taken in the investigation of this branch of the subject by De la Rive|| and others; and it is more than probable that, when the right limitations are applied, constant and definite results will here also be obtained.

1627. It is a most important part of the character of the current, and essentially connected with its very nature, that it is always the same. The two forces are everywhere in it. There is never one current of force or one fluid only. Any one part of the current may, as respects the presence of the two forces there, be considered as precisely the same with any other part; and the numerous experiments which imply their possible separation, as well as the theoretical expressions which, being used daily, assume it, are, I think, in contradiction with facts (511, &c.). It appears to me to be as impossible to assume a current of positive or a current of negative force alone, or of the two at once with any predominance of one over the other, as it is to give an absolute charge to matter (1169. 1177.).

1628. The conviction of this truth, if, as I think, it be a truth, or on the other hand the disproof of it, is of the greatest consequence. If, as a first principle, we can establish that the centres of the two forces, or elements of force, never can be separated to any sensible distance, or at all events not further than the space between two contiguous particles (1615.), or if we can establish the contrary conclusion, how much more clear is our view of what lies before us, and how much less embarrassed the ground over which we have to pass in attaining to it, than if we remain halting between two opinions! And if, with that feeling, we rigidly test every experiment which bears upon the point, as far as our prejudices will let us (1161.), instead of permitting them with a theoretical expression to pass too easily away, are we not

* See De la Rive's Researches, Bib. Universelle, 1829, xl. p. 40.

† Amongst others, Davy, Philosophical Transactions, 1821, p. 438. Pelletier's important results, Annales de Chimie, 1834, lvi. p. 371. and Becquerel's non-heating current, Bib. Universelle, 1835, lx. 218.

‡ Philosophical Transactions, 1824, pp. 225. 228.

§ Annales de Chimie, 1836, lxii. 177.

|| Bib. Universelle, 1829, xl. 49; and Ritchie, Phil. Trans. 1832, p. 296.

much more likely to attain the real truth, and from that proceed with safety to what is at present unknown?

1629. I say these things not, I hope, to advance a particular view, but to draw the strict attention of those who are able to investigate and judge of the matter, to what must be a turning point in the theory of electricity; to a separation of two roads, one only of which can be right: and I hope I may be allowed to go a little further into the facts which have driven me to the view I have just given.

1630. When a wire in the voltaic circuit is heated, the temperature frequently rises first, or most at one end. If this effect were due to any relation of positive or negative as respects the current, it would be exceedingly important. I therefore examined several such cases; but when, keeping the contacts of the wire and its position to neighbouring things unchanged, I altered the direction of the current, I found that the effect remained unaltered, showing that it depended, not upon the direction of the current, but on other circumstances. So there is here no evidence of a difference between one part of the circuit and another.

1631. The same point, i. e. uniformity in every part, may be illustrated by what may be considered as the inexhaustible nature of the current when producing particular effects; for these effects depend upon transfer only, and do not consume the power. Thus a current which will heat one inch of platina wire will heat a hundred inches (853. note). If a current be sustained in a constant state, it will decompose the fluid in one voltameter only, or in twenty others if they be placed in the circuit, in each to an amount equal to that in the single one.

1632. Again, in cases of disruptive discharge, as in the spark, there is frequently a dark part (1422.), which, by Professor Johnson, has been called the neutral point*; and this has given rise to the use of expressions implying that there are two electricities existing separately, which, passing to that spot, there combine and neutralize each other†. But if such expressions are understood as correctly indicating that positive electricity alone is moving between the positive ball and that spot, and negative electricity only between the negative ball and that spot, then what strange conditions these parts must be in; conditions, which to my mind are every way unlike that which really occurs! In such a case, one part of a current would consist of positive electricity only, and that moving in one direction; another part would consist of negative electricity only, and that moving in the other direction; and a third part would consist of an accumulation of the two electricities, not moving in either direction, but mixing up together, and being in a relation to each other utterly unlike any relation which could be supposed to exist in the two former portions of the discharge. This does not seem to me to be natural. In a current, whatever form the discharge may take, or whatever part of the circuit or current is referred to, as much positive force as is there exerted in one direction, so much negative force is there exerted in the other. If it were not so we should have bodies electrified not merely

* Silliman's Journal, 1834, xxv. p. 57.

† Thomson on Heat and Electricity, p. 471.

positive and negative, but on occasions in a most extraordinary manner, one being charged with five, ten, or twenty times as much of both positive and negative electricity in equal quantities as another. At present, however, there is no known fact indicating such states.

1633. Even in cases of convection, or carrying discharge, the statement that the current is everywhere the same must in effect be true (1627.): for how, otherwise, could the results formerly described occur? When currents of air constituted the mode of discharge between the portions of paper moistened with iodide of potassium or sulphate of soda (465. 469.), decomposition occurred; and I have since ascertained that, whether a current of positive air issued from a spot, or one of negative air passed towards it, the effect of the evolution of iodine or of acid was the same, whilst the reversed currents produced alkali. So also in the magnetic experiments (307.) whether the discharge was effected by the introduction of a wire, or the occurrence of a spark, or the passage of convective currents either one way or the other, (depending on the electrified state of the particles) the result was the same, being in all cases dependent upon the perfect current.

1634. Hence, the section of a current compared with other sections of the same current must be a constant quantity, if the actions exerted be of the same kind; or if of different kinds, then the forms under which the effects are produced are equivalent to each other, and experimentally convertible at pleasure. It is in sections, therefore, we must look for identity of electrical force, even to the sections of sparks and carrying actions, as well as those of wires and electrolytes.

1635. In illustration of the utility and importance of establishing that which may be the true principle, I will refer to a few cases. The doctrine of unipolarity as formerly stated, and I think generally understood*, is evidently inconsistent with my view of a current (1627.); and the later singular phenomena of poles and flames described by Erman and others† partake of the same inconsistency of character. If a unipolar body could exist, i. e. one that could conduct the one electricity and not the other, what very new characters we should have a right to expect in the currents of single electricities passing through them, and how greatly ought they to differ, not only from the common current which is supposed to have both electricities travelling in opposite directions in equal amount at the same time, but also from each other! The facts, which are excellent, have, however, gradually been more correctly explained by Becquerel‡, Andrews §, and others; and I understand that Professor Ohms || has perfected the work, in his close examination of all the phenomena; and

* Erman, Annales de Chimie, 1807. lxi. p. 115. Davy's Elements, p. 168. Biot, Ency. Brit. Supp. iv. p. 444. Becquerel, Traité, i. p. 167. De la Rive, Bib. Univ. 1837. vii. 392.

† Erman, Annales de Chimie, 1824. xxv. 278. Becquerel, Ibid. xxxvi. p. 329.

‡ Becquerel, Annales de Chimie, 1831. xlvi. p. 283.

§ Andrews, Philosophical Magazine, 1836. ix. 182.

|| Schweigger's Jahrbuch der Chemie, &c. 1830. Heft 8. Not understanding German, it is with extreme

after showing that similar phenomena can take place with good conductors, proves that with soap, &c. many of the effects are the mere consequences of the bodies evolved by electrolytic action.

1636. I conclude, therefore, that the *facts* upon which the doctrine of unipolarity was founded are not adverse to that unity and indivisibility of character which I have stated the current to possess, any more than the phenomena of the pile itself, which might well bear comparison with those of unipolar bodies, are opposed to it. Probably the effects which have been called effects of unipolarity, and the peculiar differences of the positive and negative surface when discharging into air, gases, or other dielectrics (1480. 1525.) which have been already referred to, may have considerable relation to each other*.

1637. M. de la Rive has recently described a peculiar and remarkable effect of heat on a current when passing between electrodes and a fluid†. It is, that if platina electrodes dip into acidulated water no change is produced in the passing current by making the positive electrode hotter or colder; whereas making the negative electrode hotter increased the deflexion of a galvanometer affected by the current, from 12° to 30° and even 45°, whilst making it colder diminished the current in the same high proportions.

1638. That one electrode should have this striking relation to heat whilst the other remained absolutely without, seem to me as incompatible with what I conceived to be the character of a current as unipolarity (1627. 1635.), and it was therefore with some anxiety that I repeated the experiment. The electrodes which I used were of platina; the electrolyte, water containing about one sixth of sulphuric acid by weight: the voltaic battery consisted of two pairs of amalgamated zinc and platina plates in dilute sulphuric acid, and the galvanometer in the circuit was one with two needles, and gave when the arrangement was complete a deflexion of 10° or 12°.

1639. Under these circumstances heating either electrode increased the current; heating both produced still more effect. When both were heated, if either were cooled, the effect on the current fell in proportion. The proportion of effect due to heating this or that electrode varied, but on the whole heating the negative seemed to favour the passage of the current somewhat more than heating the positive.

regret I confess I have not access, and cannot do justice, to the many most valuable papers in experimental electricity published in that language. I take this opportunity also of stating another circumstance which occasions me great trouble, and, as I find by experience, may make me seemingly regardless of the labours of others:—it is a gradual loss of memory for some years past; and now, often when I read a memoir, I remember that I have seen it before, and would have rejoiced if at the right time I could have recollected and referred to it in the progress of my own papers.—M. F.

* See also Hare in Silliman's Journal, 1833. xxiv. 246.

† Bibliothèque Universelle, 1837. vii. 388.

Whether the application of heat were by a flame applied underneath, or one directed by a blow pipe from above, or by a hot iron or coal, the effect was the same.

1640. Having thus removed the difficulty out of the way of my views regarding a current, I did not pursue this curious experiment further. It is probable, that the difference between my results and those of M. de la Rive may depend upon the relative values of the currents used; for I employed only a weak one resulting from two pairs of plates two inches long and half an inch wide, whilst M. de la Rive used four pairs of plates of sixteen square inches in surface.

1641. Electric discharges in the atmosphere in the form of balls of fire have occasionally been described. Such phenomena appear to me to be incompatible with all that we know of electricity and its modes of discharge. As time is an element in the effect (1418. 1436.) it is possible perhaps that an electric discharge might really pass as a ball from place to place; but as every thing shows that its velocity must be almost infinite, and the time of its duration exceedingly small, it is impossible that the eye should perceive it as anything else than a line of light. That phenomena of balls of fire may appear in the atmosphere, I do not mean to deny; but that they have anything to do with the discharge of ordinary electricity, or are at all related to lightning or atmospheric electricity, is much more than doubtful.

1642. All these considerations, and many others, help to confirm the conclusion, drawn over and over again, that the current is an indivisible thing; an axis of power, in every part of which both electric forces are present in equal amount* (517. 1627.). With conduction and electrolyzation, and even discharge by spark, such a view will harmonize without hurting any of our preconceived notions; but as relates to convection, a more startling result appears, which must therefore be considered.

1643. If two balls A and B be electrified in opposite states and held within each other's influence, the moment they move towards each other, a current, or those effects which are understood by the word current, will be produced. Whether A move towards B, or B move in the opposite direction towards A, a current, and in both cases having the same *direction*, will result. If A and B move from each other, then a *current* in the opposite direction, or equivalent effects, will be produced.

1644. Or, as charge exists only by induction (1178. 1299.), and a body when electrified is necessarily in relation to other bodies in the opposite state; so, if a ball be electrified positively in the middle of a room and be then moved in any direction, effects will be produced, as if a *current* in the same direction (to use the conventional

* I am glad to refer here to the results obtained by Mr. Christie with magneto-electricity, Philosophical Transactions, 1833, p. 113. note. As regards the current in a wire, they confirm everything that I am contending for.

mode of expression) had existed: or, if the ball be negatively electrified, and then moved, effects as if a current in a direction contrary to that of the motion had been formed, will be produced.

1645. I am saying of a single particle or of two what I have before said, in effect, of many (1633.). If the former account of currents be true, then that just stated must be a necessary result. And, though the statement may seem startling at first, it is to be considered that, according to my theory of induction, the charged conductor or particle is related to the distant conductor in the opposite state, or that which terminates the extent of the induction, by all the intermediate particles (1165. 1295.), these becoming polarized exactly as the particles of a solid electrolyte do when interposed between the two electrodes. Hence the conclusion regarding the unity and identity of the current in the case of convection, jointly with the former cases, is not so strange as it might at first appear.

1646. There is a very remarkable phenomenon or effect of the electrolitic discharge, first pointed out, I believe, by Mr. Porrett, of the accumulation of fluid under decomposing action in the current on one side of an interposed diaphragm*. It is a mechanical result; and as the liquid passes from the positive towards the negative electrode in all the known cases, it seems to establish a relation to the polar condition of the dielectric in which the current exists (1164. 1525.). It has not as yet been sufficiently investigated by experiment; for De la Rive says†, it requires that the water should be a bad conductor, as, for instance, distilled water, the effect not happening with strong solutions; whereas, Dutrochet says‡ the contrary is the case, and that, the effect is not directly due to the electric current.

1647. Becquerel in his Traité de l'Electricité has brought together the considerations which arise for and against the opinion, that the effect generally is an electric effect§. Though I have no decisive fact to quote at present, I cannot refrain from venturing an opinion, that the effect is analogous both to combination and convection (1623.), being a case of carrying due to the relation of the diaphragm and the fluid in contact with it, through which the electric discharge is jointly effected; and further, that the peculiar relation of positive and negative small and large surfaces already referred to (1482. 1503. 1525.), may be the direct cause of the fluid and the diaphragm travelling in contrary but determinate directions. A very valuable experiment has been made by M. Becquerel with particles of clay‖, which will probably bear importantly on this point.

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* Annals of Philosophy, 1816. viii. p. 75.

† Annales de Chimie, 1835. xxviii. p. 196.

‡ Annales de Chimie, 1832. xlix. p. 423.

§ Vol. iv. p. 197. 192.

‖ Traité de l'Electricité, i. p. 285.

1648. *As long as* the terms *current* and *electro-dynamic* are used to express those relations of the electric forces in which progression of either fluids or effects are supposed to occur (283.), *so long* will the idea of velocity be associated with them; and this will, perhaps, be more especially the case if the hypothesis of a fluid or fluids be adopted.

1649. Hence has arisen the desire of estimating this velocity either directly or by some effect dependent on it; and amongst the endeavours to do this correctly, may be mentioned especially those of Dr. WATSON* in 1748, and of Professor WHEATSTONE† in 1834; the electricity in the early trials being supposed to travel from end to end of the arrangement, but in the later investigations a distinction occasionally appearing to be made between the transmission of the effect and of the supposed fluid by the motion of whose particles that effect is produced.

1650. Electrolytic action has a remarkable bearing upon this question of the velocity of the current, especially as connected with the theory of an electric fluid or fluids. In it there is an evident transfer of power with the transfer of each particle of the anion or cathion present, to the next particles of the cathion or anion; and as the amount of power is definite, we have in this way a means of localizing as it were the force, identifying it by the particle and dealing it out in successive portions, which leads, I think, to very striking results.

1651. Suppose, for instance, that water is undergoing decomposition by the powers of a voltaic battery. Each particle of hydrogen as it moves one way, or of oxygen as it moves in the other direction, will transfer a certain amount of electrical force associated with it in the form of chemical affinity (822. 852. 918.) onwards through a distance, which is equal to that through which the particle itself has moved. This transfer will be accompanied by a corresponding movement in the electrical forces throughout every part of the circuit formed (1627. 1634.), and its effects may be estimated, as, for instance, by the heating of a wire (853.) at any particular section of the current however distant. If the water be a cube of an inch in the side, the electrodes touching, each by a surface of one square inch, and being an inch apart, then, by the time that a tenth of it, or 25·25 grains, is decomposed, the particles of oxygen and hydrogen throughout the mass may be considered as having moved relatively to each other in opposite directions, to the amount of the tenth of an inch; i. e. that two particles at first in combination will after the motion be the tenth of an inch apart. Other motions which occur in the fluid will not at all interfere with this result; for they have no power of accelerating or retarding the electric discharge, and possess in fact no relation to it.

1652. The quantity of electricity in 25·25 grains of water is, according to an estimate of the force which I formerly made (861.), equal to above 24 millions of charges of a large Leyden battery; or it would have kept any length of a platina wire $\frac{1}{104}$ of an inch in diameter red hot for an hour and a half (853.). This result, though given

* Philosophical Transactions, 1748.

† Ibid. 1834, p. 583.

only as an approximation, I have seen no reason as yet to alter, and it is confirmed generally by the experiments and results of M. Pouillet*. According to Mr. Wheatstone's experiments the influence or effects of the current would appear at a distance of 576,000 miles in a second†. We have, therefore, in this view of the matter, on the one hand, an enormous quantity of power equal to a most destructive thunder storm appearing instantly at the distance of 576,000 miles from its source, and on the other, a quiet effect, in producing which the power had taken an hour and a half to travel through the tenth of an inch: yet these are the equivalents to each other, being effects observed at the sections of one and the same current (1634.).

1653. It is time that I should call attention to the lateral or transverse forces of the *current*. The great things which have been achieved by Oersted, Arago, Ampere, Davy, De la Rive, and others, and the high degree of simplification which has been introduced into their arrangement by the theory of Ampere, have not only done their full service in advancing most rapidly this branch of knowledge, but have secured to it such attention that there is no necessity for urging on its pursuit. I refer of course to magnetic action and its relations; but though this is the only recognised lateral action of the current, there is great reason for believing that others exist and would by their discovery reward a close search for them (951.).

1654. The magnetic or transverse action of the current seems to be in a most extraordinary degree independent of those variations or modes of action which it presents directly in its course; it consequently is of the more value to us, as it gives us a higher relation of the power than any that might have varied with each mode of discharge. This discharge, whether it be by conduction through a wire with infinite velocity (1652.), or by electrolyzation with its corresponding and exceeding slow motion (1651.), or by spark, and probably even by convection, produces a transverse magnetic action always the same in kind and direction.

1655. It has been shown by several experimenters, that whilst the discharge is of the *same kind* the amount of lateral or magnetic force is very constant (366. 367. 368. 376.). But when we wish to compare discharge of different kinds, for the important purpose of ascertaining whether the same amount of current will in its *different forms* produce the same amount of transverse action, we find the data very imperfect. Davy noticed, that when the electric current was passing through an aqueous solution it affected a magnetic needle‡, and Dr. Ritchie says, that the current in the electrolyte is as magnetic as that in a metallic wire§, and has made water revolve round a magnet as a wire carrying the current would revolve.

1656. Disruptive discharge produces its magnetic effects: a strong spark, passed

* Becquerel, Traité de l'Electricité, v. p. 278.

† Philosophical Transactions, 1834, p. 589.

‡ Philosophical Transactions, 1821, p. 426.

§ Ibid. 1832, p. 294.

transversely to a steel needle, will magnetise it as well as if the electricity of the spark were conducted by a metallic wire occupying the line of discharge; and Sir H. DAVY has shown that the discharge of a voltaic battery in vacuo is affected and has motion given to it by approximated magnets*.

1657. Thus the three very different modes of discharge, namely, conduction, electrolyzation, and disruptive discharge, agree in producing the important transverse phenomenon of magnetism. Whether convection or carrying discharge will produce the same phenomenon has not been determined, and the few experiments I have as yet had time to make do not enable me to answer in the affirmative.

1658. Having arrived at this point in the consideration of the current and in the endeavour to apply its phenomena as tests of the truth or fallacy of the theory of induction which I have ventured to set forth, I am now very much tempted to indulge in a few speculations respecting its lateral action and its possible connexion with the transverse condition of the lines of ordinary induction (1165. 1304.). I have long sought and still seek for an effect or condition which shall be to statical electricity what magnetic force is to current electricity; for as the lines of discharge are associated with a certain transverse effect, so it appeared to me impossible but that the lines of tension or of inductive action, which of necessity precede that discharge, should also have their correspondent transverse condition or effect (951.).

1659. According to the beautiful theory of AMPERE, the transverse force of a current may be represented by its attraction for a similar current and its repulsion of a contrary current. May not then the equivalent transverse force of static electricity be represented by that lateral tension or repulsion which the lines of inductive action appear to possess (1304.)? Then again, when current or discharge occurs between two bodies, previously under inductrical relations to each other, the lines of inductive force will weaken and fade away, and, as their lateral repulsive tension diminishes, will contract and ultimately disappear in the line of discharge. May not this be an effect identical with the attractions of similar currents? i. e. may not the passage of static electricity into current electricity, and that of the lateral tension of the lines of inductive force into the lateral attraction of lines of similar discharge, have the same relation and dependencies, and run parallel to each other?

1660. The phenomena of induction amongst currents which I had the good fortune to discover some years ago (6. &c. 1048.) may perchance here form a connecting link in the series of effects. When a current is first formed, it tends to produce a current in the contrary direction in all the matter around it; and if that matter have conducting properties and be fitly circumstanced, such a current is produced. On the contrary, when the original current is stopped, one in the same direction tends to form all around it, and, in conducting matter properly arranged, will be excited.

* Philosophical Transactions, 1821. p. 427.

1661. Now though we perceive the effects only in that portion of matter which, being in the neighbourhood, has conducting properties, yet hypothetically it is probable, that the non-conducting matter has also its relations to, and is affected by, the disturbing cause, though we have not yet discovered them. Again and again the relation of conductors and non-conductors has been shown to be one not of opposition in kind, but only of degree (1334. 1603.); and, therefore, for this, as well as for other reasons, it is probable, that what will affect a conductor will affect an insulator also; producing perhaps what may deserve the term of the electrotonic state (60. 242. 1114).

1662. It is the feeling of the necessity of some lateral connexion between the lines of electric force (1114.); of some link in the chain of effects as yet unrecognised, that urges me to the expression of these speculations. The same feeling has led me to make many experiments on the introduction of insulating dielectrics having different inductive capacities (1270. 1277.) between magnetic poles and wires carrying currents, so as to pass across the lines of magnetic force. I have employed such bodies both at rest and in motion, without, as yet, being able to detect any influence produced by them; but I do by no means consider the experiments as sufficiently delicate, and intend, very shortly, to render them more decisive.

1663. I think the hypothetical question may at present be put thus: can such considerations as those already generally expressed (1658.) account for the transverse effects of electrical currents? are two such currents in relation to each other merely by the inductive condition of the particles of matter between them, or are they in relation by some higher quality and condition (1654.), which, acting at a distance and not by the intermediate particles, has, like the force of gravity, no relation to them?

1664. If the latter be the case, then, when electricity is acting upon and in matter, its direct and its transverse action are essentially different in their nature; for the former, if I am correct, will depend upon the contiguous particles, and the latter will not. As I have said before, this may be so, and I incline to that view at present, but I am desirous of suggesting considerations why it may not, that the question may be thoroughly sifted.

1665. The transverse power has a character of polarity impressed upon it. In the simplest forms it appears as attraction or repulsion, according as the currents are in the same or different directions: in the current and the magnet it takes up the condition of tangential forces; and in magnets and their particles produces poles. Since the experiments have been made which have persuaded me that the polar forces of electricity, as in induction and electrolytic action (1298. 1343.), show effects at a distance only by means of the polarized contiguous and intervening particles, I have been led to expect that *all polar forces* act in the same general manner; and the other kinds of phenomena which one can bring to bear upon the subject seem fitted to strengthen that expectation. Thus in crystallizations the effect is transmitted from

particle to particle; and in this manner, in acetic acid or freezing water a crystal a few inches or even a couple of feet in length will form in less than a second, but progressively and by a transmission of power from particle to particle. And, as far as I remember, no case of polar action, or partaking of polar action, except the one under discussion, can be found which does not act by contiguous particles*. It is apparently of the nature of polar forces that such should be the case, for the one force either finds or developes the contrary force near to it, and has, therefore, no occasion to seek for it at a distance.

1666. But leaving these hypothetical notions respecting the nature of the lateral action out of sight, and returning to the direct effects, I think that the phenomena examined and reasoning employed in this and the two preceding papers tend to confirm the view first taken (1164.), namely, that ordinary inductive action and the effects dependent upon it, are due to an action of the contiguous particles of the dielectric interposed between the charged surfaces or parts which constitute, as it were, the terminations of the effect. The great point of distinction and power (if it have any) in the theory is, the making the dielectric of essential and specific importance, instead of leaving it as it were a mere accidental circumstance or the simple representative of space, having no more influence over the phenomena than the space occupied by it. I have still certain other results and views respecting the nature of the electrical forces and excitation, which are connected with the present theory; and, unless upon further consideration they sink in my estimation, I shall very shortly put them into form as another series of these electrical researches.

* I mean by contiguous particles those which are next to each other, not that there is *no* space between them. See (1616.).

Royal Institution,
February 14*th,* 1838.

METEOROLOGICAL JOURNAL,

KEPT BY THE ASSISTANT SECRETARY,

AT THE APARTMENTS OF THE

ROYAL SOCIETY,

BY ORDER OF

THE PRESIDENT AND COUNCIL.

OBSERVANDA.

Height of the Cistern of the Barometer above the plinth at Waterloo Bridge....83 feet 2 inches.
________________ above the mean level of the sea ....97 feet.
Height of the receiver of the Rain Gauge above the court of Somerset House ..79 feet.
The External Thermometer is 2 feet higher than the Barometer Cistern.
The Thermometers are graduated to Fahrenheit's scale.
The Barometer is divided into inches and tenths.
The Hours of Observation are of Mean Time, the day beginning at Midnight.
The *daily* observations of the Barometer are *not* corrected.
The *monthly means* are corrected for capillarity and temperature by the Table contained in Mr. Baily's paper in *Phil. Trans.* for 1837.

METEOROLOGICAL JOURNAL FOR JULY AND AUGUST, 1837.

1837.	9 o'clock, A.M. Barometer uncorrected. Flint Glass.	9 o'clock, A.M. Barometer uncorrected. Crown Glass.	9 o'clock, A.M. Att. Ther.	3 o'clock, P.M. Barometer uncorrected. Flint Glass.	3 o'clock, P.M. Barometer uncorrected. Crown Glass.	3 o'clock, P.M. Att. Ther.	Dew Point at 9 A.M. deg. Fahr.	Diff. of Wet and Dry Bulb Thermometer.	External Thermometers. Fahrenheit. 9 A.M.	External Thermometers. Fahrenheit. 3 P.M.	External Thermometers. Self-registering. Lowest	External Thermometers. Self-registering. Highest	Rain in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
JULY															
S 1	30.336	30.328	62.8	30.316	30.312	63.3	50	06.7	56.7	62.3	50.0	75.3		E	A.M. Cloudy—light brisk wind. P.M. Fine—light clouds and wind. Evening, Fine and clear.
● ☉ 2	30.332	30.326	65.2	30.240	30.236	63.5	52	07.1	58.0	69.7	47.5	65.2		ENE	Fine—light clouds and haze throughout the day. Evening, Cloudy.
M 3	30.240	30.234	69.7	30.156	30.150	66.6	56	08.6	65.3	70.5	57.0	72.7		E	Fine—nearly cloudless—lt. haze throughout the day. Evening, Cldy.
T 4	30.142	30.136	65.3	30.128	30.122	67.0	58	07.2	64.6	70.4	55.7	75.0		NE	A.M. Cloudy—light breeze. P.M. Fine—light clouds. Evening, Cloudy.
W 5	30.180	30.172	66.7	30.128	30.120	66.9	53	09.8	63.4	69.8	55.3	71.5		SSE	A.M. Cloudy—light breeze. P.M. Overcast. Evening, Light rain.
T 6	30.150	30.144	67.8	30.136	30.130	68.3	60	08.7	65.6	70.3	60.6	72.3	.033	ESE	Overcast throughout the day.
F 7	30.252	30.248	64.2	30.240	30.234	67.2	57	04.3	59.5	68.7	55.6	72.5		NE	A.M. Overcast—light wind. P.M. Fine—light clouds. Evening, Fine and clear.
S 8	30.196	30.192	67.3	30.144	30.136	68.8	60	08.0	67.3	75.4	57.7	70.0		SW	Fine—nearly cloudless throughout the day. Evening, Overcast.
☉ 9	30.164	30.156	66.0	30.154	30.150	65.8	50	07.2	57.9	60.7	51.6	76.4		E	A.M. Fine—light clouds—light brisk wind. P.M. Cloudy—light wind. Evening, Fine and clear.
M 10	29.970	29.964	65.7	29.926	29.920	64.9	54	06.9	61.4	69.8	50.0	63.9		E	A.M. Fine—light clouds and wind. P.M. Fine—nearly cloudless. Evening, Fine and clear.
T 11	29.992	29.986	65.2	29.958	29.950	65.7	56	05.9	61.5	67.8	52.0	70.0		E	Fine—nearly cloudless—lt. breeze throughout the day. Ev. Cloudy.
W 12	29.958	29.950	61.7	29.916	29.908	63.6	54	04.3	57.7	63.5	54.8	70.2		E	Overcast—light wind throughout the day.
T 13	29.818	29.812	62.5	29.804	29.796	63.9	56	05.9	62.5	65.8	56.9	64.2		S	Overcast—light breeze throughout the day.
F 14	29.776	29.770	64.6	29.786	29.782	66.6	57	04.7	63.8	71.3	58.7	69.5		S	A.M. Overcast—brisk wind. P.M. Cloudy. 2¼ P.M. Heavy shower. Evening, Overcast—occasional showers. 7 P.M. Rainbow visible.
S 15	29.894	29.888	66.9	29.868	29.860	67.0	61	05.0	65.4	62.5	59.3	72.2	.250	S var.	A.M. Fine—dark threatening clouds—light wind. P.M. Overcast—heavy rain. Evening, Fine and clear.
☉ 16	29.994	29.990	68.2	29.992	29.984	66.8	59	07.8	64.8	65.6	54.7	73.2	.355	SSW	A.M. Cloudy—light rain and wind. P.M. Overcast—light rain—distant thunder. Evening, Fine and clear.
○ M 17	30.044	30.040	69.8	29.994	29.990	67.5	58	08.3	64.5	68.5	53.5	72.2	.041	SW	A.M. Fine—lt. clouds & breeze. P.M. Cloudy. Ev. Ovct.—lt. rain.
T 18	29.866	29.860	65.2	29.828	29.820	67.2	60	05.8	64.3	69.5	58.7	70.8	.158	SW	A.M. Cloudy—light rain and wind. P.M. Cloudy—light breeze. Evening, Very light rain.
W 19	29.852	29.846	67.2	29.804	29.798	68.2	59	07.2	65.4	71.3	59.2	71.4		SSW	A.M. Cloudy—lt. wind. P.M. Fine—lt. clds. Ev. Overcast—lt. rain.
T 20	29.900	29.896	68.0	29.922	29.916	67.8	60	07.8	64.8	63.3	58.0	72.0	.047	W	A.M. Cloudy—light breeze. P.M. Overcast—light rain. Evening, Continued rain.
F 21	30.050	30.044	70.8	30.034	30.028	67.3	57	07.3	63.4	68.3	55.8	67.3	.102	WNW	Fine—light clouds and wind throughout the day. Evening, Cloudy—very light rain.
S 22	30.088	30.082	66.0	30.058	30.052	67.3	57	06.2	63.3	71.7	56.5	69.8		NW	Fine—light clouds & breeze throughout the day. Ev. Fine & clear.
☉ 23	30.058	30.052	68.7	30.000	29.996	70.0	62	06.9	68.4	76.5	59.9	72.2		SSW	A.M. Fine—lt. clouds. P.M. Cloudy. Ev. Overcast—very light rain.
M 24	30.146	30.142	74.2	30.126	30.120	71.4	57	07.7	63.7	71.2	59.0	78.3		NE	A.M. Fine—dark clouds—light breeze. P.M. Fine—light clouds. Evening, Cloudy.
T 25	30.126	30.120	71.0	30.108	30.102	71.0	60	08.8	67.0	72.3	61.3	72.3		W	Fine—light clouds & breeze throughout the day. Evening, Cloudy.
W 26	30.096	30.090	70.3	30.084	30.078	71.3	62	08.0	69.3	74.3	64.2	73.7		S	A.M. Fine—nearly cloudless. P.M. Cloudy. Evening, Fine & clear.
T 27	30.018	30.012	73.3	29.904	29.898	72.9	61	09.2	70.6	77.4	60.3	78.5		SSW	A.M. Fine—lt. clouds. P.M. Fine—nearly cloudless—stiff breeze. Evening, Cloudy.
F 28	29.818	29.812	71.4	29.796	29.790	72.5	63	07.4	68.5	73.6	62.3	79.6		SW	A.M. Fine—lt. clds. & breeze. P.M. Cloudy—lt. rain. Ev. Overcast—light rain & wind.
S 29	29.350	29.344	67.2	29.288	29.278	69.9	60	04.4	64.3	66.4	60.2	74.2	.255	SE var.	A.M. Ovt.—lt. rain, very h. wind. Sudden fall in Bar. 11 A.M. Heavy shr. P.M. Ovt.—occasional shrs. Ev. Ovt.—lt. rain—cont. h. wind.
☉ 30	29.544	29.540	65.6	29.640	29.636	67.7	58	06.3	61.7	66.8	57.0	68.7	.091	SW	A.M. Overcast—wind abated, although very high during night. P.M. Cloudy. Ev. Overcast—occasional showers, with lightning.
M 31	29.924	29.922	69.6	29.890	29.886	67.4	56	07.2	62.8	68.6	53.7	68.3	.102	SW	A.M. Fine—lt. clouds. P.M. Cloudy, with occasional rain. Ev. Cldy.
MEAN.	30.009	30.003	67.4	29.980	29.973	67.6	57.5	06.9	63.8	69.2	56.7	71.7	Sum. 1.434		Mean Barometer corrected .... 9 A.M. F. 29.909 .. 3 P.M. 29.879; 9 A.M. C. 29.902 .. 3 P.M. 29.871
AUGUST															
● T 1	29.752	29.746	64.3	29.698	29.692	64.9	58	05.1	59.7	60.2	56.9	70.0	.036	SE	Overcast—light rain and wind throughout the day. Ev. High wind.
W 2	29.694	29.688	64.7	29.672	29.666	67.4	60	04.2	64.5	68.2	57.4	64.8	.422	S var.	Overcast—light rain with high wind nearly all the day. Evening, Continued rain and wind.
T 3	29.716	29.712	69.2	29.708	29.702	68.5	61	05.1	65.0	68.7	59.9	71.0	.033	S var.	A.M. Cloudy—light rain—high wind. P.M. Fine—light clouds—high wind. Evening, Overcast—light rain.
F 4	29.896	29.892	68.2	29.948	29.944	68.0	60	07.0	64.3	68.3	57.2	69.5	.080	SW	A.M. Overcast—light rain and wind. P.M. Cloudy—4 P.M. heavy shower—6½ P.M. rainbow. Evening, Overcast, occasional shrs.
S 5	30.182	30.176	67.5	30.194	30.186	66.3	56	08.8	61.3	65.3	52.2	69.3	.375	SW	Fine—light clouds and wind throughout the day. Ev. Fine & clear.
☉ 6	30.266	30.258	66.8	30.250	30.246	66.0	55	06.9	60.7	66.0	52.0	66.0		E	Fine—light clouds and wind throughout the day. Evening, Cloudy.
M 7	30.356	30.350	62.0	30.372	30.368	64.3	57	05.5	60.8	64.3	53.2	66.3		E	Overcast—light brisk wind throughout the day. Evening, Cloudy.
T 8	30.396	30.392	65.5	30.338	30.332	64.5	55	08.0	61.7	65.8	51.5	65.6		NE	A.M. Cloudy—light brisk wind. P.M. Fine—light clouds & wind. Evening, Overcast.
W 9	30.142	30.136	67.2	30.032	30.024	65.2	57	06.8	62.9	69.8	54.0	66.4		NE var.	A.M. Fine—light clouds—brisk wind. P.M. Cloudy—light wind.
T 10	29.968	29.962	63.3	29.916	29.910	66.2	59	04.6	62.0	72.0	57.3	70.2		E	A.M. Overcast—light breeze. P.M. Fine—light clouds and wind. Evening, Overcast—very light rain.
F 11	29.884	29.878	65.8	29.916	29.908	67.8	61	05.7	65.2	71.4	61.3	72.0		NNW	A.M. Lightly Overcast. P.M. Fine—lt. clouds & wind. Ev. Overcast.
S 12	30.016	30.010	68.0	29.994	29.988	68.6	61	06.1	65.3	72.5	60.3	72.0		SW	A.M. Lightly overcast. P.M. Fine—light clouds. Ev. Fine & clear.
☉ 13	30.160	30.154	66.2	30.172	30.168	68.5	60	05.6	63.4	74.5	56.9	73.4		S	A.M. Overcast. P.M. Fine—light clouds. Evening, Cloudy.
M 14	30.286	30.282	70.3	30.258	30.252	70.2	61	07.5	68.3	75.4	59.6	74.6		E	Fine—nearly cloudless throughout the day.
T 15	30.278	30.274	71.0	30.210	30.204	70.6	60	07.2	65.0	73.3	56.5	77.0		E	A.M. Fine and clear—light breeze. P.M. Cloudy—light wind. Evening, Fine and clear.
○ W 16	30.140	30.132	67.3	30.068	30.064	69.7	61	04.8	65.2	69.4	58.5	75.0		E	Overcast—light rain with wind. Evening, Fine and clear.
T 17	30.044	30.038	68.5	30.010	30.006	71.5	66	02.8	67.5	75.8	62.5	74.0	.022	E	A.M. Overcast. P.M. Fine—light clouds. Evening, Fine and clear.
F 18	30.160	30.156	69.0	30.162	30.158	72.0	65	05.5	65.5	76.2	62.2	78.8	.008	SW	A.M. Overcast—light wind. P.M. Fine—light clouds.
S 19	30.126	30.122	72.0	30.022	30.018	73.0	66	04.0	67.5	76.4	63.3	76.8		NNW	Overcast—light wind throughout the day. Evening, Fine—light clouds and wind.
☉ 20	29.938	29.934	80.2	29.954	29.950	74.0	68	06.4	70.7	73.4	63.5	77.5		SW var.	A.M. Fine—light clouds and wind. P.M. Cloudy—brisk wind.
M 21	30.100	30.096	70.0	30.086	30.082	72.2	65	07.1	67.3	72.0	61.0	75.0		SW	Overcast—brisk wind throughout the day. Ev. Cloudy—light rain.
T 22	30.172	30.168	70.0	30.150	30.146	71.0	67	07.0	67.5	72.5	59.0	73.5		SSW	A.M. Cloudy—brisk wind. P.M. Overcast. Evening, Heavy shower.
W 23	30.054	30.050	69.2	30.042	30.038	71.0	65	04.0	65.8	71.0	65.2	73.6	.444	W	Overcast—light rain nearly the whole of the day. Ev. Heavy rain.
T 24	30.212	30.208	63.5	30.204	30.200	66.5	52	05.7	56.8	63.2	55.0	71.9	.638	N	A.M. Overcast—brisk wind. P.M. Fine—light clouds. Evening, Fine & clear.
F 25	30.228	30.224	62.0	30.148	30.146	64.0	57	05.0	58.5	66.0	50.4	63.3		E	A.M. Overcast. P.M. Fine—light clouds and wind.
S 26	29.888	29.884	62.7	29.854	29.850	64.5	62	05.0	64.5	64.6	56.0	67.0	.022	SSW	A.M. Overcast—distant thunder and lightning—heavy rain. P.M. Overcast—heavy rain.
☉ 27	30.180	30.176	64.3	30.190	30.186	62.0	50	02.7	53.5	59.9	48.5	72.7	.763	E	Fine—light clouds—brisk wind nearly the whole of the day.
M 28	30.044	30.040	62.0	29.878	29.874	63.0	54	05.2	58.5	62.8	49.7	60.0		E	A.M. Cloudy. P.M. Fine—light clouds. Evening, Overcast.
T 29	29.504	29.500	60.0	29.460	29.456	61.0	54	00.8	55.5	59.4	53.2	65.5	.116	NE	Overcast—light rain throughout the day.
W 30	29.402	29.398	59.0	29.478	29.476	60.3	55	01.5	54.4	57.4	52.6	59.5	.602	NNW	Overcast, with occasional showers. Evening, Overcast—thunder and lightning.
T 31	29.456	29.450	60.8	29.414	29.408	60.5	54	02.4	56.4	62.2	49.2	59.3	.041	SSW	Fine—light clouds throughout the day. Evening, Fine and clear.
MEAN.	30.021	30.016	66.5	29.993	29.989	67.2	59.7	05.7	62.7	68.3	56.6	70.0	Sum. 3.602		Mean Barometer corrected .... 9 A.M. F. 29.923 .. 3 P.M. 29.894; 9 A.M. C. 29.917 .. 3 P.M. 29.889

METEOROLOGICAL JOURNAL FOR SEPTEMBER AND OCTOBER, 1837.

1837.	9 o'clock, A.M. Barometer uncorrected. Flint Glass.	9 o'clock, A.M. Barometer uncorrected. Crown Glass.	9 o'clock, A.M. Att. Ther.	3 o'clock, P.M. Barometer uncorrected. Flint Glass.	3 o'clock, P.M. Barometer uncorrected. Crown Glass.	3 o'clock, P.M. Att. Ther.	Dew Point at 9 A.M., deg. Fahr.	Diff. of Wet and Dry Bulb Thermometer.	External Thermometers. Fahrenheit. 9 A.M.	External Thermometers. Fahrenheit. 3 P.M.	External Thermometers. Self-registering. Lowest	External Thermometers. Self-registering. Highest	Rain at inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
SEPTEMBER															
F 1	29.368	29.362	57.4	29.386	29.382	59.5	53	05.8	56.3	59.8	50.7	62.3		W	A.M. Overcast—light wind. P.M. Fine—light clouds and wind. Evening, Cloudy—light rain.
S 2	29.444	29.438	58.2	29.532	29.526	59.9	52	00.8	55.4	59.7	51.2	62.0	.091	N	A.M. Cloudy—light brisk wind. P.M. Fine—light clouds. Evening, Cloudy—light rain.
☉ 3	29.666	29.660	59.8	29.704	29.700	59.0	49	05.1	53.4	55.8	50.3	61.2	.072	W	Overcast—light wind nearly the whole day. Evening, Light rain.
M 4	29.920	29.914	56.7	29.934	29.928	58.6	50	03.5	53.0	58.2	51.2	57.3	.055	WNW	Overcast—light wind throughout the day. Evening, Fine and clear.
T 5	29.864	29.808	57.3	29.912	29.906	58.6	52	04.0	56.6	61.7	49.3	59.7		E	A.M. Fine—light clouds and wind. P.M. Cloudy—light rain and wind. Evening, Cloudy.
W 6	30.060	30.052	55.3	30.010	30.002	57.4	50	03.7	52.0	60.7	47.6	61.6		N	A.M. Thick haze—light wind. P.M. Fine—nearly cloudless. Evening, Fine and clear.
T 7	29.946	29.942	58.8	29.872	29.866	60.2	52	04.4	59.3	63:3	51.3	61.6		S	A.M. Fine—light clouds—brisk wind. P.M. Overcast—brisk wind. Evening, Light rain.
F 8	29.930	29.924	59.6	29.932	29.926	60.5	54	02.5	55.5	61.9	54.5	66.0	.158	SE	A.M. Overcast—lt. rain. P.M. Fine—lt. clouds. Evening, Overcast.
S 9	29.872	29.866	61.7	29.734	29.730	62.5	55	05.6	60.8	66.8	55.5	62.2		SSE	A.M. Lightly overcast—light wind. P.M. Fine—light clouds—brisk wind. Evening, Overcast—light rain—high wind.
☉10	29.916	29.910	64.4	29.940	29.936	64.0	56	03.7	60.2	67.2	54.7	67.3	.055	SSW	Fine—light clouds and wind throughout the day. Evening, Fine and clear.
M11	29.822	29.818	64.0	29.718	29.714	64.0	59	04.0	62.5	64.4	56.2	68.0		SE var.	A.M. Fine—light clouds—brisk wind. P.M. Overcast—brisk wind. Evening, Heavy rain.
T 12	29.748	29.746	60.0	29.634	29.630	63.0	55	05.0	54.4	61.0	52.0	67.0	.277	SW	Overcast—light wind throughout the day. Evening, Fine and clear.
W13	29.154	29.150	61.0	29.100	29.096	62.6	58	04.5	59.8	63.0	52.2	61.6	.052	S	A.M. Cloudy—light wind. P.M. Overcast—light wind.
○T 14	29.354	29.350	58.0	29.468	29.464	61.0	53	04.5	56.0	59.2	53.0	64.0	.069	NNW	A.M. Overcast—light wind. P.M. Lightly cloudy—brisk wind.
F15	29.634	29.630	59.0	29.750	29.746	58.6	52	04.5	55.5	58.4	49.8	59.5	.055	NW	A.M. Lightly overcast—light wind. P.M. Fine—light clouds and wind. Evening, Fine and clear.
S 16	30.004	30.000	56.0	29.964	29.960	57.4	52	05.0	55.0	60.6	48.5	60.0	.011	SE var.	Overcast—brisk wind throughout the day. Evening, Fine and clear—brisk wind.
☉17	30.058	30.054	59.5	30.064	30.060	62.0	58	05.5	65.5	67.7	56.0	66.0		W var.	Lightly cloudy—brisk wind throughout the day. Evening, Overcast—high wind.
M18	30.052	30.048	62.8	30.026	30.020	64.2	60	04.8	64.6	65.7	61.5	69.7		SSW	Overcast—brisk wind throughout the day.
T19	30.170	30.164	63.2	30.122	30.116	65.0	60	05.9	63.4	67.8	58.8	68.3		SSE	Overcast—light wind throughout the day. Evening, Fine and clear.
W20	30.028	30.022	65.6	29.948	29.942	65.6	61	06.0	66.3	65.2	61.2	68.5		E	Fine—nearly cloudless—lt. wind throughout the day. Ev. Fine & clear.
T21	29.934	30.928	62.2	29.958	29.950	64.9	56	02.7	59.2	65.6	54.3	68.4		NE	A.M. Cloudy—light brisk wind. P.M. Fine—nearly cloudless—light wind. Evening, Fine and clear.
F 22	30.086	30.080	63.0	30.068	30.062	64.8	58	03.6	60.5	64.2	53.2	68.7		NE	A.M. Cloudy—light brisk wind. P.M. Fine—nearly cloudless—light wind. Evening, Fine and clear.
S 23	30.144	30.136	64.8	30.130	30.122	62.0	55	06.4	58.3	60.2	52.9	65.2		E	Fine and cloudless—light brisk wind nearly the whole of the day. Evening, Fine and clear.
☉24	30.242	30.236	62.0	30.244	30.240	59.5	50	07.3	56.7	57.3	51.8	61.2		NE	Fine—lt. clouds—brisk wind throughout the day. Evening, Cloudy.
M25	30.320	30.314	58.3	30.302	30.296	58.3	51	06.2	56.2	58.2	46.5	58.4		N	Fine—nearly cloudless—light wind. Evening, Fine and clear.
T26	30.302	30.296	57.0	30.238	30.230	58.9	50	05.2	54.2	56.2	46.4	59.3		NE	A.M. Fine—nearly cloudless—light wind. P.M. Overcast—light rain. Evening, Cloudy.
W27	30.116	30.110	54.9	30.074	30.066	57.2	49	03.1	51.2	57.6	44.4	60.2		N	Cloudy—light wind the whole of the day. Evening, Overcast.
T28	30.076	30.072	55.3	30.040	30.034	58.5	50	04.2	53.4	59.3	44.4	60.2		N	A.M. Cloudy—light brisk wind. P.M. Fine—lt. clouds. Evening, Cloudy.
● F 29	30.064	30.058	55.8	30.008	30.004	56.8	50	03.9	54.9	58.7	44.4	60.2		NE	Fine—nearly cloudless—light wind. Evening, Cloudy.
S 30	29.972	29.968	55.8	29.946	29.942	57.9	51	02.9	57.3	61.7	48.0	59.6		NE	A.M. Fine—nearly cloudless. P.M. Fine—lt. clouds. Ev. Cloudy.
MEAN.	29.909	29.902	59.6	29.892	29.887	60.7	53.7	04.5	57.6	61.6	51.7	63.2	Sum. .895		Mean Barometer corrected .... 9 A.M. 3 P.M. F. 29.830 .. 29.810; C. 29.822 .. 29.804
OCTOBER															
☉ 1	29.928	29.924	57.8	29.930	29.926	61.3	53	04.2	57.4	62.2	48.2	62.6	.055	NE	Overcast—light fog throughout the day, with occasional rain.
M 2	30.158	30.152	60.7	30.176	30.170	62.3	58	04.2	62.2	66.5	48.0	63.8	.025	SSW	A.M. Lightly overcast—very light rain. P.M. Fine—light clouds. Evening, Fine and clear.
T 3	30.174	30.166	60.0	30.096	30.090	62.3	58	04.1	59.3	65.8	54.0	67.2		SE	A.M. Thick fog. P.M. Fine—light clouds and wind. Evening, Overcast—light rain.
W 4	30.048	30.042	62.2	30.080	30.074	64.4	58	05.0	63.2	65.4	54.0	67.3		SW	A.M. Lightly overcast. P.M. Fine—light clouds and wind. Evening, Fine and clear.
T 5	30.264	30.256	61.2	30.246	30.240	62.8	57	04.8	57.4	61.7	52.7	68.5		WSW	A.M. Light fog. P.M. Cloudy—light wind. Evening, Overcast.
F 6	30.088	30.082	60.8	30.092	30.086	62.3	57	03.1	59.3	63.4	55.0	61.8	.361	SSW	A.M. Overcast—heavy rain. P.M. Fine—light clouds and wind. Evening, Fine and starlight.
S 7	30.246	30.240	59.4	30.228	30.220	60.8	53	01.3	53.7	62.0	49.2	63.4	.022	SW	A.M. Fine—very light fog and wind. P.M. Cloudy.
☉ 8	30.204	30.198	59.0	30.126	30.122	60.0	54	04.6	58.3	60.0	52.7	62.0		SSW	Lightly overcast nearly the whole of the day, with light rain. Evening, Fine and starlight.
M 9	30.304	30.298	57.2	30.306	30.300	58.4	51	04.4	53.8	57.6	47.4	61.5	.033	W	Fine—light clouds throughout the day. Evening, Fine & starlight.
T10	30.364	30.356	56.5	30.354	30.348	59.3	51	04.5	55.5	60.3	47.9	60.0		SW	A.M. Overcast—light wind. P.M. Fine—lt. clouds. Evening, Cloudy.
W11	30.424	30.418	57.0	30.402	30.396	58.8	52	03.9	56.4	62.2	52.8	61.2		SW	A.M. Cloudy—light wind. P.M. Fine—lt. clouds. Ev. Fine & clear.
○T12	30.470	30.464	56.4	30.478	30.472	58.6	50	03.2	52.4	59.6	49.2	62.7		SW	A.M. Fine—nearly cloudless—very light fog. P.M. Cloudy—light wind. Evening, Fine & clear.
F13	30.596	30.590	53.0	30.588	30.582	55.2	44	04.3	47.7	56.2	43.8	60.5		N	A.M. Fine & cloudless—light brisk wind. P.M. Cloudy—lt. wind.
S 14	30.708	30.700	52.4	30.672	30.664	53.4	45	03.5	47.2	54.3	43.9	56.5		N	Fine—nearly cloudless—light wind nearly the whole of the day. Evening, Foggy.
☉15	30.640	30.634	50.8	30.562	30.558	52.0	44	02.6	44.2	54.3	38.5	55.6		W	A.M. Fine—very light fog. P.M. Fine—light clouds. Ev. Foggy.
M16	30.488	30.480	50.6	30.402	30.392	52.4	44	04.3	47.7	54.7	44.0	54.6		SW	A.M. Light fog. P.M. Overcast. Evening, Fine and clear.
T17	30.268	30.260	50.0	30.174	30.168	52.4	44	02.5	44.6	55.2	41.0	55.5		SSW	A.M. Light fog. P.M. Overcast. Evening, Very light rain.
W18	30.112	30.104	52.2	30.120	30.112	53.7	46	03.8	52.0	57.8	44.3	56.2		SW	A.M. Fine—light clouds. P.M. Cloudy—lt. wind. Ev. Overcast.
T19	30.400	30.396	51.3	30.408	30.402	53.2	44	02.6	45.2	55.2	44.0	58.4		S	A.M. Light fog. P.M. Fine—light clouds. Evening, Cloudy.
F20	30.538	30.532	52.2	30.538	30.532	54.5	48	03.2	51.7	62.0	45.0	56.0		SW	A.M. Fine—light clouds—thick fog early. P.M. Cloudy.
S 21	30.640	30.632	53.8	30.588	30.580	54.9	49	03.8	52.4	55.7	50.9	62.3		N	A.M. Light fog. P.M. Overcast. Evening, Cloudy.
☉22	30.456	30.450	53.2	30.362	30.358	55.0	47	03.0	49.4	57.8	47.0	56.2		SW	Cloudy—light fog and wind throughout the day. Evening, Overcast—light mist.
M23	30.046	30.040	54.2	29.894	29.888	56.2	49	04.9	54.7	59.4	49.2	58.3		SSW	A.M. Cloudy—light wind. P.M. Fine—light clouds. Evening, Overcast—heavy rain.
T24	29.594	29.590	54.7	29.494	29.490	54.8	49	02.2	50.4	50.2	50.2	60.2	.388	SSW	A.M. Overcast—heavy rain. P.M. Continued rain.
W25	29.700	29.694	50.6	29.846	29.840	51.4	42	02.0	42.4	48.7	38.3	52.7	.250	N	Overcast—light rain—light brisk wind throughout the day.
T26	30.062	30.054	47.6	29.964	29.956	52.3	40	02.8	42.3	51.6	35.4	49.4	.061	SW	A.M. Fine—lt. clouds & wind. P.M. Lightly overcast. Ev. H. wind.
F27	29.486	29.480	51.4	29.540	29.534	52.2	48	02.0	53.4	51.4	42.0	54.2		S var.	A.M. Overcast—light rain—high wind—high wind during the night. P.M. Fine—light clouds and wind. Evening, Cloudy.
● S28	29.610	29.602	48.8	29.428	29.420	50.3	42	03.9	47.3	49.7	40.8	54.0		S	A.M. Fine—light clouds and wind. P.M. Overcast—occasional rain. Evening, Continued rain.
☉29	29.458	29.452	47.7	29.440	29.436	49.0	40	02.3	41.2	48.0	38.8	51.3	.477	SW	Fine—lt. clouds & wind during the day. Ev. Overcast—very lt. rain.
M30	29.350	29.348	47.5	29.218	29.216	50.7	42	04.6	48.2	57.5	38.7	51.3	.041	S	Overcast—light brisk wind, with occasional rain throughout the day.
T31	29.356	29.350	49.2	29.406	29.400	49.9	43	02.8	45.0	47.3	43.2	58.2	.108	SW	Fine—nearly cloudless—light wind throughout the day. Evening, Overcast—high wind.
MEAN.	30.135	30.129	54.2	30.102	30.096	56.0	48.4	03.5	51.5	57.2	46.1	58.8	Sum. 1.821		Mean Barometer corrected .... 9 A.M. 3 P.M. F. 30.070 .. 30.032; C. 30.063 .. 30.025

METEOROLOGICAL JOURNAL FOR NOVEMBER AND DECEMBER, 1837.

1837.	9 o'clock, A.M. Barometer uncorrected. Flint Glass.	9 o'clock, A.M. Barometer uncorrected. Crown Glass.	9 o'clock, A.M. Att. Ther.	3 o'clock, P.M. Barometer uncorrected. Flint Glass.	3 o'clock, P.M. Barometer uncorrected. Crown Glass.	3 o'clock, P.M. Att. Ther.	Dew Point at 9 A.M. deg. Fahr.	Diff. of Wet and Dry Bulb Ther.	External Thermometers. Fahrenheit. 9 A.M.	External Thermometers. Fahrenheit. 3 P.M.	External Thermometers. Self-registering Lowest	External Thermometers. Self-registering Highest	Rain in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
NOVEMBER															
W 1	28.978	28.974	48.8	28.780	28.774	51.3	45	02.7	52.4	55.8	41.4	54.3	.311	SSW	Overcast—light rain—high wind throughout the day. Very high wind throughout the night.
T 2	29.130	29.126	48.4	29.166	29.160	49.4	42	03.0	42.8	45.6	41.0	56.4	.150	W	A.M. Overcast—lt. rain—h. wind. P.M. Cldy—lt. wind. Ev. Lt. rain.
F 3	29.258	29.252	45.3	29.320	29.314	47.0	37	02.3	39.7	45.8	36.4	48 8	.036	SW	A.M. Cloudy—light fog and wind. P.M. Fine—light clouds. Evening, Light fog, with occasional flashes of lightning.
S 4	29.694	29.688	43.3	29.844	29.836	45.4	38	02.1	40.2	47.5	36.3	48.3		SW	Fine—light clouds, with very light fog throughout the day. Evening, Fine and starlight.
☉ 5	30.072	30.064	42.8	30.074	30.068	45.0	36	02.4	39.8	47.4	36.8	48.3		WSW	A.M. Cloudy—light fog. P.M. Overcast. Evening, Light fog.
M 6	30.314	30.306	42.4	30.348	30.342	43.8	33	04.4	38.7	45.8	36.6	48.3		NW	A.M. Fine—light fog and wind. P.M. Fine—light clouds and wind. Evening, Light fog.
T 7	30.456	30.450	41.4	30.406	30.400	42.6	33	01.8	34.7	44.8	32.8	46.8		E	A.M. Thick fog. P.M. Fine—nearly cloudless. Evening, Foggy.
W 8	30.320	30.312	39.3	30.246	30.238	39.7	32	01.5	32.0	38.6	30.4	45.3		E	A.M. Thick fog—white frost. P.M. Dense fog.
T 9	30.142	30.136	37.6	30.058	30.052	40.3	30	02.4	30.7	45.0	28.0	40.3		E	A.M. Light fog—sharp frost. P.M. Overcast—very light mist.
F 10	29.984	29.978	42.6	29.920	29.912	44.6	38	02.6	46.2	52.8	31.0	47.8	.019	WSW	Overcast—deposition nearly the whole day. Ev. Very light rain.
S 11	29.940	29.934	47.6	29.964	29.958	49.3	42	03.0	48.4	52.5	45.8	54.3	.050	WSW	Fine—light clonds and wind nearly the whole day. Ev. Overcast.
○☉ 12	30.018	30.012	47.3	30.098	30.090	47.3	40	03.5	43.7	44.9	42.8	54.3		WSW	A.M. Fine—nearly cloudless—light wind. P.M. Fine and cloudless. Evening, Fine and clear.
M 13	30.118	30.110	44.0	30.006	30.000	45.2	37	02.4	40.3	45.7	36.7	46.0		WSW	A.M. Light fog and wind. P.M. Overcast—light rain. Evening, Heavy rain—high wind.
T 14	29.552	29.546	46.8	29.474	29.468	47.7	43	03.1	46.3	46.4	40.0	49.3	.286	W	A.M. Fine—nearly cloudless. P.M. Overcast—light rain.
W 15	29.948	29.942	43.8	30.018	30.010	44.5	37	02.7	38.6	42.3	36.8	48.7		N	Fine—light clouds & wind throughout the day. Ev. Fine—lt. clouds.
T 16	30.008	30.002	41.8	29.946	29.940	42.3	35	01.8	37.0	40.3	35.4	44.5		N	A.M. Light fog. P.M. Overcast. Evening, Light fog.
F 17	29.980	29.974	40.3	30.008	30.000	41.6	34	02.6	35.6	41.7	32.7	41.3		N	Fine—light clouds and wind throughout the day—slight frost during the night. Evening, Fine and clear.
S 18	30.128	30.122	39.0	30.058	30.052	39.9	32	02.0	34.6	40.3	31.4	42.3		S	Light fog throughout the day—white frost during night. Ev. Overcast.
☉ 19	30.010	30.004	41.9	29.900	29.896	44.6	37	03.2	44.8	51.5	33.2	45.5		SW	A.M. Lightly overcast—deposition. P.M. Fine—light clouds. Evening, Overcast—heavy rain.
M 20	29.872	29.864	45.8	29.762	29.754	47.0	42	02.2	44.7	48.3	42.7	52.6	.200	SSW	Fine—light clouds and wind nearly the whole of the day. Evening, Overcast—very light raln.
T 21	29.850	29.844	44.3	29.864	29.856	45.8	38	02.6	39.7	46.9	36.5	49.0		SSW	A.M. Fine—nearly cloudless. P.M. Fine—lt. clouds. Ev. Overcast.
W 22	29.930	29.926	45.7	29.900	29.896	47.9	42	02.7	50.6	54.7	39.8	51.3	.033	SW	A.M. Overcast—very light rain and wind. P.M. Overcast—light wind. Evening, High wind.
T 23	29.806	29.800	50.6	29.714	29.708	51.6	48	03.1	52.4	53.7	50.3	55.2		S var.	Overcast—high wind throughout the day, and also night. Ev. Lt. rain.
F 24	29.954	29.948	51.2	29.960	29.954	51.3	46	02.0	46.7	48.6	46.4	55.0	.088	SW	Light fog and wind during the whole of the day. Evening, Overcast—light rain.
S 25	30.092	30.086	47.3	30.156	30.150	47.3	40	02.4	39.3	42.4	39.2	49.0		N	Fine—light clouds & wind throughout the day. Ev. Fine & clear.
☉ 26	30.070	30.062	43.2	29.714	29.710	45.0	36	02.5	37.8	45.5	33.4	43.3		SW	A.M. Lightly overcast—light wind. P.M. Overcast—light rain. Evening, Heavy rain.
M 27	29.524	29.518	43.2	29.494	29.488	44.2	37	01.9	37.8	43.3	37.2	48.7	.233	SW	Fine—nearly cloudless, with light fog nearly the whole day. Evening, Overcast.
● T 28	29.282	29.276	43.7	29.214	29.208	44.5	39	02.3	42.2	43.0	37.9	45.0		WSW	A.M. Light fog and wind. P.M. Overcast—very light rain.
W 29	29.494	29.488	41.3	29.634	29.626	41.8	34	02.4	34.7	40.3	33.4	45.7	.013	W	A.M. Fine—nearly cloudless—very light fog. P.M. Fine—light clouds. Evening, Fine and clear.
T 30	29.700	29.694	40.4	29.634	29.626	42.8	36	02.6	42.2	49.3	33.3	45.7		S	A.M. Overcast—light mist. P.M. Cloudy. Evening, Overcast—light rain.
MEAN.	29.854	29.848	44.0	29.823	29.816	45.4	38.0	02.5	41.2	46.4	37.2	49.0	Sum. 1.419	Mean Barometer corrected	9 A.M. 3 P.M. F. 29.817 .. 29.782 C. 29.809 .. 29.774
DECEMBER															
F 1	29.884	29.878	45.8	30.000	29.994	46.3	40	01.9	42.4	46.8	41.8	50.8	.038	SW	A.M. Fine—nearly cloudless—very light fog. P.M. Fine—light clouds. Evening, Fine and starlight.
S 2	30.212	30.204	43.2	30.298	30.292	43.3	36	02.0	36.4	41.4	36.0	47.7		SW	Thick fog throughout the day.
☉ 3	30.428	30.420	41.4	30.412	30.408	42.0	35	01.4	37.2	40.2	35.7	42.6		ENE	Overcast—deposition—light wind throughout the day. Ev. Foggy.
M 4	30.500	30.492	39.4	30.484	30.476	39.7	31	01.6	33.2	38.5	32.4	41.3		N	A.M. Thick fog—white frost. P.M. Overcast. Evening, Light fog.
T 5	30.372	30.366	39.8	30.286	30.278	39.8	33	02.7	37.8	38.8	33.3	40.3		NE	A.M. Light fog and wind. P.M. Overcast.
W 6	30.126	30.120	39.7	29.996	29.992	39.6	33	02.7	37.9	36.4	36.8	40.0		N	A.M. Light fog and wind. P.M. Overcast—light snow and wind. Evening, Light rain.
T 7	29.794	29.788	37.8	29.752	29.748	38.4	32	00.4	34.3	37.6	31.7	38.9	.061	NE	A.M. Overcast—light snow and wind. P.M. Overcast—light rain.
F 8	29.668	29.660	39.9	29.626	29.620	40.6	35	01.5	37.0	38.6	34.5	39.0	.075	NNE	Overcast throughout the day, with occasional rain.
S 9	29.690	29.686	39.3	29.732	29.726	39.6	34	01.7	35.4	37.8	34.7	41.0	.019	N	Overcast—light wind throughout the day. Evening, Very light mist.
☉ 10	30.024	30.016	39.3	30.016	30.012	40.0	34	01.2	36.2	40.2	34.6	40.2		N	Overcast—light wind throughout the day, with light rain.
M 11	30.110	30.104	39.2	30.092	30.086	39.9	33	03.0	37.9	38.8	36.0	41.5	.027	N	Overcast—light wind throughout the day. Evening, Light fog.
○ T 12	29.994	29.988	39.3	29.892	29.886	39.7	33	02.8	34.7	38.3	34.6	40.2		W	A.M. Overcast—light fog and wind. P.M. Lightly overcast. Evening, Light fog and rain.
W 13	30.040	30.032	39.2	30.114	30.106	40.3	34	01.7	36.4	42.2	34.9	39.0		SW	A.M. Thick fog. P.M. Fine—lt. clouds. Ev. Light fog—deposition.
T 14	30.256	30.250	39.9	30.230	30.224	41.4	34	02.2	37.2	42.5	36.0	43.0		SE	A.M. Light fog. P.M. Fine—light clouds. Evening, Overcast.
F 15	30.156	30.150	41.0	30.064	30.058	41.3	37	02.3	37.7	38.8	36.6	43.4		SSE	Fine—light clouds and wind throughout the day. Ev. Overcast.
S 16	29.886	29.880	40.2	29.792	29.786	41.6	35	02.2	38.8	45.3	37.0	40.4		ESE	A.M. Fine—nearly cloudless. P.M. Fine—light clouds. Evening, Overcast—light rain.
☉ 17	29.808	29.802	44.0	29.720	29.716	46.0	40	01.4	46.8	49.5	38.7	48.2	.063	SE	Overcast—deposition the whole day. Ev. Heavy rain, with high wind.
M 18	29.362	29.354	47.3	29.416	29.410	49.0	46	01.9	50.5	51.7	46.5	51.4	.191	SW	A.M. Heavy rain, with high wind. High wind throughout the night. P.M. Fine—light clouds. Evening, Light rain, with high wind.
T 19	29.976	29.970	49.0	29.928	29.922	49.6	44	01.7	45.2	50.5	45.2	53.6	.166	WSW	A.M. Fine—light clouds. Very high wind the early part of the night. P.M. Overcast—light rain.
W 20	29.560	29.554	51.2	29.316	29.310	52.0	46	02.1	53.2	52.3	45.0	54.2	.286	SW	A.M. Overcast—deposition—light wind. P.M. Light rain and snow. Evening, Very high wind.
T 21	29.964	29.956	48.3	30.160	30.152	47.7	42	02.6	42.2	43.0	42.0	55.2	.055	N	A.M. Cloudy—h. wind. P.M. Overcast—lt. brisk wind. Ev. High wind.
F 22	30.100	30.094	48.7	29.974	29.966	49.6	44	02.1	44.9	48.7	41.0	45.7		SSW	A.M. Cloudy—light wind. P.M. Overcast—very light rain and wind. Evening, Overcast.
S 23	29.942	29.936	50.4	29.948	29.942	50.8	47	01.7	49.4	50.2	44.9	52.7		W	A.M. Light fog—deposition. P.M. Overcast. Evening, Overcast.
☉ 24	30.020	30.012	49.5	29.890	29.886	50.0	45	02.4	47.3	52.5	46.0	50.8		SSW	A.M. Overcast—very light rain. P.M. Overcast—deposition.
M 25	29.796	29.790	51.0	29.868	29.864	52.0	47	02.8	52.2	54.4	47.0	53.3		SW var.	A.M. Fine—heavy clouds—brisk wind. P.M. Fine—light clouds. Evening, Fine and starlight.
T 26	29.950	29.944	50.2	29.876	29.870	50.8	45	01.5	47.6	48.8	45.2	55.3		S	Overcast—deposition—light wind throughout the day.
● W 27	29.806	29.800	48.7	29.822	29.814	50.2	44	02.0	44.5	47.8	43.6	50.4		ENE	Overcast—light fog nearly the whole of the day.
T 28	29.896	29.892	49.4	29.902	29.896	50.3	45	02.2	48.8	49.7	44.3	49 7		S	A.M. Lightly overcast—light brisk wind. P.M. Overcast—light wind.
F 29	29.860	29.852	49.2	29.836	29.830	49.9	45	02.4	47.5	48.4	44 3	50.7		SE	A.M. Fine—light clouds and wind. P.M. Fine—nearly cloudless. Evening, Overcast.
S 30	29.966	29.960	49.9	29.956	29.950	50.9	47	02.5	49 8	50.8	47.0	50.7		SE	A.M. Overcast—light wind. P.M. Fine—light clouds and wind. Evening, Very light mist.
☉ 31	29.994	29.988	49.6	29.996	29.990	51.0	45	02.1	46.7	48.7	46.3	51.8		SW	A.M. Fine—light clouds. P.M. Lightly overcast. Evening, Fine and starlight.
MEAN.	29.972	29.966	44.5	29.948	29.942	45.3	39.4	02.0	42.2	44.8	39.8	46.6	Sum. .981	Mean Barometer corrected	9 A.M. 3 P.M. F. 29.933 .. 29.907 C. 29.926 .. 29.900

PHILOSOPHICAL

TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

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FOR THE YEAR MDCCCXXXVIII.

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A Copley Medal to Professor K. F. Gauss, Forn. Memb. R.S., for his "Inventions and Mathematical Researches on Magnetism."

Another Copley Medal to Michael Faraday, Esq., D.C.L., F.R.S., for his "Researches on Specific Electrical Induction."

The Royal Medal, in the department of Chemistry, to Thomas Graham, Esq., M.A., F.R.S., for his paper entitled "Inquiries respecting the Constitution of Salts, of Oxalates, Nitrates, Phosphates, Sulphates, and Chlorides," published in the Philosophical Transactions for 1836.

The Royal Medal, in the department of Mathematics, to William Henry Fox Talbot, Esq., F.R.S., for his papers entitled "Researches in the Integral Calculus," published in the Philosophical Transactions for 1836 and 1837.

PHILOSOPHICAL TRANSACTIONS.

IX. THE BAKERIAN LECTURE.—*On the Theory of the Astronomical Refractions.* By
JAMES IVORY, *K.H. M.A. F.R.S. L. & E., Instit. Reg. Sc. Paris, Corresp. et Reg. Sc. Gottin. Corresp.*

Received April 26,—Read May 3, 1838.

THE apparent displacement of the stars caused by the inflection of light in its passage through the atmosphere, is treated by the astronomer like most other irregularities which he has occasion to consider. A set of mean quantities is first provided; and the occasional deviations of the true places from the mean are ascertained and corrected according to the state of the air, as indicated by the meteorological instruments. The subject of the astronomical refractions is thus resolved into two parts very distinct from one another; the first embracing the mean refractions, which are an unchangeable set of numbers, at least at every particular observatory; the second relating to the temporary variations occasioned by the fluctuations which are incessantly taking place in the condition of the atmosphere. It is the first of these two questions chiefly, or that regarding the mean refractions, of which it is proposed to treat in this paper.

In order to form a just notion of the mean refractions, we may suppose that some particular star is selected, and assiduously observed for a course of time so considerable as to comprehend every possible change in the condition of the atmosphere; all these observed places being severally reduced to some assumed state of the thermometer and barometer, and being combined so as to eliminate occasional irregularities, will determine the mean refraction of the star. In this procedure it is supposed, what experience confirms, that the result will ultimately be the same for the same altitude above the horizon, provided the observations are numerous enough, and extend over a sufficient length of time. We may instance the star α Lyræ observed by Dr. Brinkley; his observations are forty-four in number, extending over five years; and the greatest deviation of single observations from the mean quantity may be stated at $\pm 20''$. The supplementary Table, extending from 85° to 89°½ of zenith distance, published in BESSEL's *Tabulæ Regiomontanæ*, is one of mean refractions calculated from many

observations at every altitude. The Table, in the same work, extending to 85° of distance from the zenith, which the supplementary one is intended to complete, may likewise be considered as having the authority of actual observation; for although a theoretical formula was used in the calculations, yet the results have been carefully corrected by a comparison both with the observations of Bradley and with those made with very perfect instruments in the observatory over which Bessel presides. These two make together a table of mean refractions of the highest authority; and being free from hypothetical admissions, to speak with precision, they form the only table of the kind of which astronomy in its actual state can boast.

The mean refractions, being a fixed set of numbers at any proposed observatory, are independent of temporary changes in the state of the air. If the general constitution of the atmosphere were so well known as to enable us to deduce the temperature, the density, and the pressure at any given altitude, from the observed condition of the air at the earth's surface, it might be possible to pitch upon an atmosphere intermediate between the extreme cases, in which the irregularities would compensate one another. From such an atmosphere the mean refractions used in astronomy might be correctly computed. But in reality we have no exact knowledge of the variations to which the air is subject in ascending above the surface of the earth. The diffusion of heat and aqueous vapour, the laws which regulate the density and pressure, are but slightly and hypothetically known. Many laborious researches in the lower part of the atmosphere, to which access can be had with instruments, have not been attended with complete success; and they have thrown no light upon what takes place in the upper parts. The limit of the atmosphere, or the height at which the air ceases to have power to refract light, is uncertain, and is no doubt, as well as the figure of the limiting surface, subject to continual fluctuation. Reflecting on what is said, it must be evident that the mean refraction of a star, which is a fixed quantity, cannot possibly be deduced from an atmosphere daily and hourly varying in its essential properties.

A table of refractions, such as is used in astronomy, contains only mean effects of the atmosphere, that take place at a given point of the earth's surface; and they should properly be compared with other mean effects at the same place. Of these mean effects a principal one is the height that must be ascended in the air for depressing the thermometer one degree, from which another mean effect is easily deduced, namely, the rate at which the density of the air decreases as the height increases. The values of these quantities, as occasionally determined at any particular place, will vary according to the actual state of the air; but a multitude of particular determinations embracing every vicissitude of the atmosphere, will at length lead to mean quantities which are constant, and such as would be observed in the same atmosphere that produces the mean refractions.

It is found that the refractive power of air depends on the density to which it is proportional; and hence the rate at which the density varies at the earth's surface

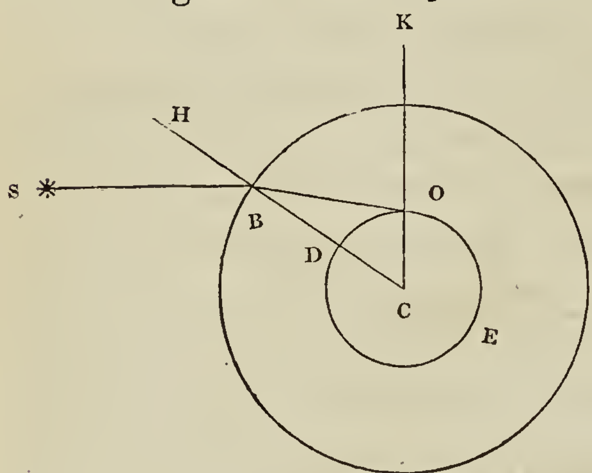
must have a great influence on the quantity of the astronomical refractions. It furnishes a key to the scale of the real densities in the atmosphere. When a thermometer is elevated in the air, it is found that the mercury continues to be depressed equably to great heights: in like manner the decrements of density will vary slowly from being proportional to the spaces passed through; so that a great share of that part of the astronomical refraction which depends upon the constitution of the atmosphere, must be ascribed to the initial rate at which the density decreases. This rate is not hypothetical; it is a real quantity independent of every other; its mean value, which alone we consider, is as determinate and as much the result of experiment as is the refractive power of the air: and in a solution of the problem which is not warped by arbitrary suppositions, and which deduces the effect only from causes really existing in nature, the former quantity will produce a part of the refraction as certain and unalterable, although perhaps not so considerable, as the latter.

But although the initial rate of the decrease of density is an essential element of the astronomical refractions, it may not alone be sufficient for a complete solution of the problem. In ascending to great heights above the earth's surface, the decrements of density will at length cease to be proportional to the spaces passed through, or to the variations of temperature. The refraction of light by the atmosphere is a complicated effect depending upon different considerations: but the influence of these considerations on the mean refractions must be uniform and free from fluctuation, and can arise only from quantities which are constant in their mean values at any proposed observatory. In speaking of mean quantities we exclude whatever is hypothetical, and confine our attention to such only as have a real existence in nature, although it may not in all cases be possible to obtain exact measurements by direct observation. As the refractions themselves are capable of being determined experimentally, they may be made the means of ascertaining what is left unknown in the formula for computing them; and they may thus contribute indirectly to advance our knowledge of the constitution of the atmosphere.

In proceeding to treat of this problem according to the notions that have been briefly explained, it remains to add that the mean effects of the atmosphere at the same observatory (of which mean effects a table of refractions is one) are alone considered, without at all entering on the question whether such effects are different or not, at different points of the earth's surface. It is very well known that the refractions, to a considerable distance from the zenith, depend only on the refractive power of the air and the spherical figure of the atmosphere: so far there is no reason to doubt that they are the same over a great part of the surface of the globe, according to the opinion generally held by astronomers: but, at greater zenith distances, when the manner in which the atmosphere is constituted comes into play, it is not so clear that they may not be subject to vary in different climates, and at different localities of the same climate. If a table of refractions at a given observatory contain a set of fixed numbers, these must be deducible from quantities not liable to change, that is,

from certain mean effects produced by the atmosphere at the observatory. To trace the relations that necessarily subsist between the mean effects that take place at a given point on the surface of the earth, is the proper business of geometry: if this can be successfully accomplished, the astronomical refractions will be made to depend upon a small number of quantities really existing in nature, and which can be determined, either directly or indirectly, by actual observation.

1. The foundation of the theory of the astronomical refractions was laid by DOMINIQUE CASSINI. The earth being supposed a perfect sphere, he conceived that it was environed by a spherical stratum of air uniform in its density from the bottom to the top. By these assumptions the computation of the refractions is reduced to a problem of the elementary geometry requiring only that there be known the height of the homogeneous atmosphere, and the refractive power of air. Let the light of a star



S fall upon the atmosphere at B, from which point it is refracted to the eye of an observer at O on the earth's surface D O E: the centre of the earth being at C, draw the radii C O K, C D B H: the angle K O B = θ, is the apparent zenith distance of the star; and O B C = φ is the angle in which the light of the star is refracted on entering the atmosphere: now from the triangle O B C we deduce

$$\sin OBC = \sin KOB \times \frac{CO}{CB};$$

or, which is the same thing, putting $i = \frac{DB}{CD}$,

$$\sin \varphi = \frac{\sin \theta}{1 + i}.$$

Again, φ being the angle in which the light of the star is refracted, if we put $\delta\theta$ for the refraction, the angle of incidence S B H, which in the present case is always greater than the angle of refraction, will be $= \varphi + \delta\theta$; and $\frac{\sin(\varphi + \delta\theta)}{\sin\varphi}$ will be a constant ratio represented by $\frac{1}{\sqrt{1 - 2\alpha}}$; so that

$$\sin(\varphi + \delta\theta) = \frac{\sin\varphi}{\sqrt{1 - 2\alpha}} = \frac{\sin\theta}{(1 + i)\sqrt{1 - 2\alpha}}.$$

Thus we have the two following equations, which furnish a very easy rule for computing the mean refractions according to CASSINI's method, viz.

$$\sin\varphi = \frac{\sin.\theta}{1 + i}$$

$$\sin(\varphi + \delta\theta) = \frac{\sin\theta}{(1 + i)\sqrt{1 - 2\alpha}}.$$

As i and α are both very small numbers, if we put

$$m = i - i^2,$$

$$n = i - \alpha - i^2 + \alpha i - \frac{3\alpha^2}{2},$$

the two last equations will become

$$\sin \varphi = \sin \theta - m \sin \theta,$$

$$\sin (\varphi + \delta \theta) = \sin \theta - n \sin \theta:$$

and by employing the usual formula for deducing the variation of the arc from the variation of the sine, we get

$$\varphi = \theta - m \tan \theta + \frac{m^2}{2} \tan^3 \theta,$$

$$\varphi + \delta \theta = \theta - n \tan \theta + \frac{n^2}{2} \tan^3 \theta:$$

consequently

$$\delta \theta = (m - n) \tan \theta - \frac{m^2 - n^2}{2} \cdot \tan^3 \theta;$$

that is,

$$\delta \theta = \left(\alpha - i\alpha + \frac{3\alpha^2}{2}\right) \tan \theta - \left(i\alpha - \frac{\alpha^2}{2}\right) \tan^3 \theta;$$

or, which is the same thing,

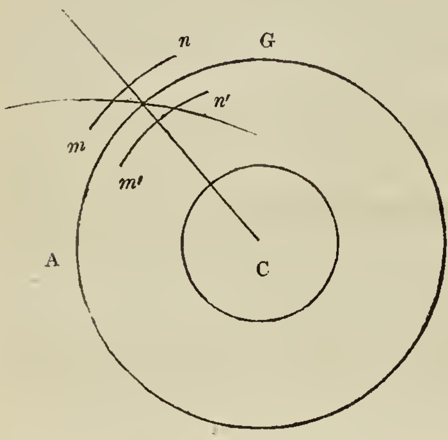
$$\delta \theta = \alpha \tan \theta \left(1 + \alpha - \frac{i - \frac{1}{2}\alpha}{\cos^2 \theta}\right) = \alpha (1 + \alpha) \tan \theta \left(1 - \frac{i - \frac{1}{2}\alpha}{\cos^2 \theta}\right),$$

agreeing exactly with LAPLACE's formula employed in computing the first part of the Table of mean refractions published by the French Board of Longitude.

2. The publication of NEWTON's Principia enabled geometers to take a more enlarged view of the astronomical refractions, and one approaching nearer to nature. According to CASSINI, the atmosphere is a spherical stratum of air, uniform in its density throughout, diffused round the earth to the height of about five miles; in reality the density decreases gradually in ascending, and is hardly so much attenuated as to be ineffective to refract the light at the great elevation of fifty miles. The path described by the light of a star in its passage through the atmosphere is therefore not a straight line, as it would be in the hypothesis of CASSINI, but a curve more and more inflected towards the earth's centre by the successive action of air of increasing density. Now in the Principia there is found whatever is necessary for determining the nature of this curve, and consequently for solving the problem of the astronomical refractions, which consists in ascertaining the difference between the direction of the light when it enters the atmosphere, and its ultimate direction when it arrives at the earth's surface. In the last section of the first book of his immortal work, NEWTON teaches in what manner the molecules of bodies act upon the rays of light and refract them; and as the atmosphere must be uniform in its condition at all equal altitudes, its action upon light can only be a force directed to the centre of the earth; so that

the trajectory in which the light moves, being described by a centripetal force, the determination of its figure will fall under the propositions contained in the second section of the same book.

Conceive that light falls upon an atmosphere A G K, constituted as CASSINI supposed, spherical in its form, concentric to the earth, of the same density ϱ throughout; and suppose that the attractive force of the molecules of air situated in the surface A G K extends to $m\ n$ on one side, and to $m'\ n'$ on the other. Every molecule of light when it arrives at $m\ n$ will be attracted by the air in a direction perpendicular to the surface A G K, and tending to C the centre of the earth; it will continue to suffer a varied attraction till it penetrates to the other surface $m'\ n'$; but when it has passed this limit, it will no longer be acted upon effectively by the surrounding air, which will attract it equally in all opposite directions.



As the attraction of air extends only to insensible distances, in estimating its action upon a molecule of light we may consider the limiting surfaces $m\ n$ and $m'\ n'$ as parallel planes, the forces being perpendicular to $m\ n$, and of the same intensity at all equal distances from it. The law of the forces in action between $m\ n$ and $m'\ n'$ is indetermined; it may be uniform, or varied in any manner. These things being premised, it follows from a fundamental proposition of the philosophy of NEWTON, the demonstration of which it would be useless to repeat here, that the total action of all the forces between $m\ n$ and $m'\ n'$ is to add to the square of the velocity of the light incident at $m\ n$, an increment which is always the same, whatever be the direction in which the light arrives at $m\ n$. If we now put v for the velocity with which the light enters $m\ n$, and v' for the velocity with which it leaves $m'\ n'$, what is said will be expressed by this equation,

$$v'^2 - v^2 = 2 \cdot \varphi(\varrho),$$

$\varphi(\varrho)$ denoting the sum of all the forces between $m\ n$ and $m'\ n'$, each multiplied by the space through which it acts, a sum which, in different atmospheres, will vary only when ϱ varies.

It will be convenient to have a name for the function $\varphi(\varrho)$, and the most appropriate term seems to be, the refractive power of the air. In using this term, or in expressing by $\varphi(\varrho)$ the action of air upon light, it is always supposed that the light passes out of a vacuum into air of the density ϱ.

A property resulting from what is said may be mentioned. Having drawn a radius from the centre of the earth to the point at which the light falls upon the atmosphere, let ϖ denote the angle made by the direction of the velocity v with the radius, and ϖ' the angle made by the direction of the velocity v' with it; then $v \sin \varpi$ and $v' \sin \varpi'$ will be the partial velocities of the light parallel to the surface of the atmosphere. Now these are equal; for all the forces which change v into v' are perpendicular to

the surface of the atmosphere, and therefore they have no effect to alter the velocity of the light parallel to that surface. Thus

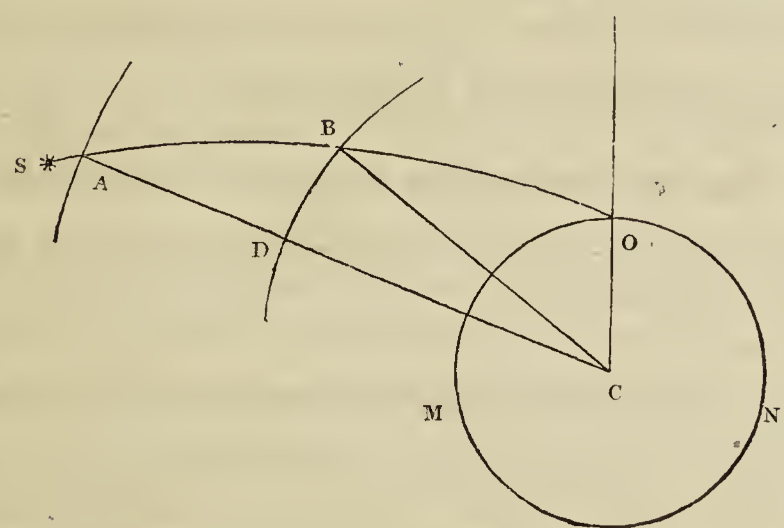
$$v \sin \varpi = v' \sin \varpi',$$

and

$$\frac{\sin \varpi}{\sin \varpi'} = \frac{v'}{v},$$

that is, in words, the ratio of the sine of incidence to the sine of refraction is equal to the ratio of the velocity of the light after refraction to the velocity of the incident light; which ratio, being independent of the direction of the incident light, is constant for all light that falls upon the atmosphere with the same velocity.

What has been said of an atmosphere supposed homogeneous is next to be applied to the real atmosphere of the earth, the density of which decreases continually in



ascending. The sphere M O N of which C is the centre, representing the earth, let S A B O be the trajectory described by light emanating from the star S, in its passage through the atmosphere to the earth's surface at O: through any two points of this curve, A and B, draw spherical surfaces concentric to the earth, the distances A C and D C from the common centre being $r + d\,r$ and r. Representing by ϱ the density of the air above the spherical surface at A, let $\varrho + d\,\varrho$ stand for the density, supposed uniform, of the stratum between the two surfaces at A and B: and it is to be observed that, though A D $= d\,r$ is an infinitesimal, it is nevertheless to be accounted infinitely great when compared to the insensible distance at which the molecular action of the air at A ceases to act: from which it follows that the refractive power of the stratum upon light which enters at A, is exactly equal to the refractive power of a homogeneous atmosphere, snpposing the density $\varrho + d\,\varrho$ to extend unvaried to the earth's surface. Now if v denote the velocity with which the light moves in the trajectory at A, the refractive power of the air above the stratum will diminish v^2 by the quantity $2\,\varphi\,(\varrho)$; for it is obvious that the refractive power of the air above the spherical surface at A, is equal and opposite to the refractive power of a homogeneous atmosphere within the same surface and of the density ϱ: on the other hand the refractive power of the stratum will augment v^2 by the quantity $2\,\varphi\,(\varrho + d\,\varrho)$:

wherefore, upon the whole, the real increment of v^2 will be $2\,\varphi\,(\varrho + d\,\varrho) - 2\,\varphi\,(\varrho)$; so that we shall have

$$d\,.\,v^2 = 2\,\varphi\,(\varrho + d\varrho) - 2\,\varphi\,(\varrho) = 2\,d\,.\,\varphi\,(\varrho);$$

and, by integrating,

$$v^2 = n^2 + 2\,\varphi\,(\varrho).$$

It is obvious that n^2 is the square of the velocity of the light before it arrives at the atmosphere, that is, when it moves in a vacuum. We may consider n^2 as the unit in parts of which the squares of the velocity of the light at the several points of the trajectory are estimated; which requires that the formula be thus written,

$$v^2 = 1 + 2\,\varphi\,(\varrho).$$

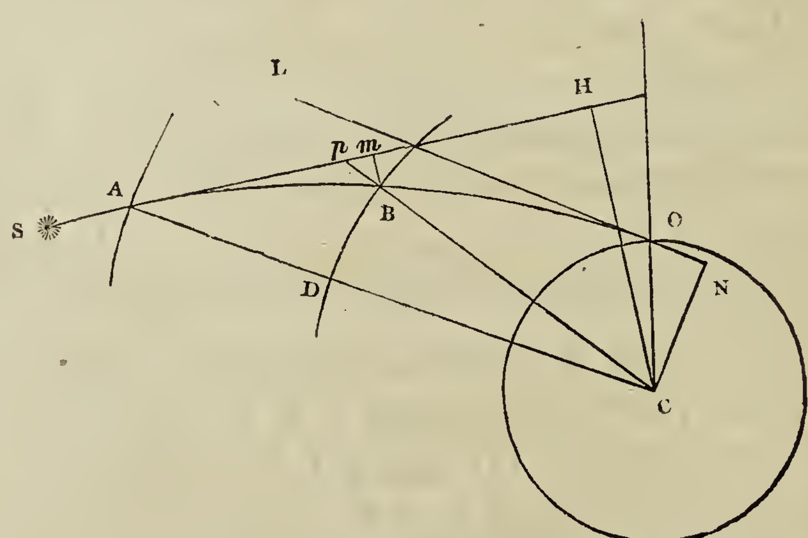
Resuming the equation

$$d\,.\,v^2 = 2\,d\,.\,\varphi\,(\varrho),$$

we have

$$v\,d\,v = d\,.\,\varphi\,(\varrho) = \frac{d\,.\,\varphi\,(\varrho)}{d\,r}\,.\,d\,r;$$

from which we learn, that the same addition which v^2 receives by the refractive power of the air at A, it will acquire by the accumulated action of the force $\frac{d\,.\,\varphi\,(\varrho)}{d\,r}$ at all the points of the line $d\,r$, or, which is the same thing, by the action of the force $\frac{d\,.\,\varphi\,(\varrho)}{d\,r}$ urging the light towards the earth's centre at all the points of the curve A B. Thus the path of the light of a star in its passage through the atmosphere is a trajectory described by the action of the centripetal force $-\frac{d\,.\,\varphi\,(\varrho)}{d\,r}$ tending to the centre of the earth, the sign $-$ being necessary, because the analytical expression is essentially negative.



Draw A H a tangent of the curve at A, B m perpendicular to A H, and produce C B to meet A H in p: put n for the angle A C O which the radius vector A C makes with C O the vertical of the observer; $d\,z$ for A B the element of the curve; $d\,\tau$ for the time of moving through A B; and R for the radius of curvature

at A. Now Bp is the space through which the centripetal force $-\frac{d.\varphi(\varrho)}{dr}$ would cause a molecule of light to move from a state of rest in the time $d\tau$: wherefore

$$2\,Bp = -\frac{d.\varphi(\varrho)}{dr} \cdot d\tau^2;$$

also

$$Bp = \frac{Bm}{\sin BAD} = \frac{dz^2}{2R} \cdot \frac{dz}{r\,dn};$$

and, by equating the equal quantities, we get

$$\frac{dz}{R} = -\frac{d.\varphi(\varrho)}{dr} \cdot \frac{d\tau^2}{dz^2} \cdot r\,dn. \quad \ldots\ldots\ldots\ldots \quad (1.)$$

The refraction of the light in moving from A to B, or the difference of the directions of the curve at A and B, is evidently equal to the angle subtended by A B at the centre of the circle of curvature, that is, to $\frac{dz}{R}$: wherefore if $\delta\,\theta$ represent the refraction increasing from the top of the atmosphere to the earth's surface, we shall have

$$d.\delta\,\theta = -\frac{d.\varphi(\varrho)}{dr} \cdot \frac{d\tau^2}{dz^2} \cdot r\,dn.$$

This formula is merely an application of the 6th proposition of the first book of the Principia.

Another general and useful expression of the differential of the refraction is easily obtained. Draw $CH = y$, perpendicular to the tangent A H: from the known properties of curve-lines, we have

$$R = \frac{r\,dr}{dy}:$$

wherefore

$$\frac{dz}{R} = dy \cdot \frac{dz}{dr} \cdot \frac{1}{r} = \frac{dy}{\sqrt{r^2 - y^2}}:$$

consequently

$$d.\delta\,\theta = \frac{dy}{\sqrt{r^2 - y^2}}, \quad \ldots\ldots\ldots\ldots\ldots \quad (2.)$$

but in this formula $\delta\,\theta$ must be conceived to increase from the surface of the earth to the top of the atmosphere.

In applying the last formula it is necessary to have a value of y. Draw O L to touch the curve at O, and C N perpendicular to O L: put ϱ', v' for the density of the air, and the velocity of the light at O; also y' for the perpendicular C N, a for C O the radius of the earth, and θ for angle C O N, which is the apparent zenith-distance of the star: we shall have

$$\frac{\text{Area } ABC}{d\tau} = \frac{dz}{d\tau} \times y = v \times y:$$

and because the curve is described by a centripetal force tending to C, the value of

$\upsilon \times y$ will be the same at all the points of the curve; wherefore

$$\upsilon \times y = \upsilon' \times y';$$

and

$$y = y' \times \frac{\upsilon'}{\upsilon} = a \sin \theta \times \frac{\upsilon'}{\upsilon}.$$

Now, according to what was before shown,

$$\upsilon = \sqrt{1 + 2\varphi(\varrho)},$$

$$\upsilon' = \sqrt{1 + 2\varphi(\varrho')};$$

wherefore

$$y = a \sin \theta \times \sqrt{\frac{1 + 2\varphi(\varrho')}{1 + 2\varphi(\varrho)}}. \quad \ldots \ldots \ldots \ldots \quad (3.)$$

By substituting this expression in the differential of the refraction, the problem will be reduced to an integration.

The equations that have been investigated are perfectly general, and will apply in any constitution of the atmosphere that may be adopted. It has been thought better to consider the manner in which the forces act, than to employ functions with peculiar properties to express the molecular action. When the light in passing through the atmosphere arrives at a surface of increased density, it receives an impulse which may be considered as instantaneous; and this impulse being distributed over the breadth of a stratum of uniform density, ascertains the centripetal force tending to the earth's centre, by the action of which the trajectory is described.

3. It appears that NEWTON himself was the first to apply this new method to the problem of the astronomical refractions. A table, which he had computed, and which he gave to Dr. HALLEY, is published in the Philosophical Transactions for 1721. Nothing is said as to the manner in which the table was constructed: and it has always been a curious and interesting question among astronomers, whether it is the result of theory, or is deduced from observations alone, without the aid of theory. Some original letters of NEWTON to FLAMSTEED, published in 1835 at the expense of the Board of Admiralty, have put an end to all doubts on this point. These letters prove that NEWTON studied profoundly the problem of the refractions; that for several months in succession he was occupied almost entirely in repeated attempts to overcome the difficulties that occurred in this research; during which time he calculated several tables with great labour, namely, the one he gave to HALLEY, and another, or rather three others, which have been found in his letters to FLAMSTEED lately printed.

Admitting that the refractive power of the air is proportional to its density, which NEWTON had established in his Optics on speculative principles, and which HAWKSBEE afterwards was the first to demonstrate experimentally, the mathematical solution of the problem requires a knowledge of the law according to which the densities vary in the atmosphere. In his first attempt NEWTON assumes that the densities decrease in ascending in the same proportion that the distances from the earth's centre increase. Now a and r denoting the same things as before, put l for the total height

of the atmosphere; then $\varphi(\varrho)$ the refractive power of the air at the distance r from the centre of the earth, will, according to this hypothesis, be expressed by the formula

$$\varphi(\varrho) = \varphi(\varrho') \times \frac{l - r + a}{l}.$$

If this value be substituted in the formula (1.), which is a deduction from the sixth proposition of the first book of the Principia, the result will be

$$d \,.\, \delta\theta = \frac{\varphi(\rho')}{l} \cdot \frac{d\tau^2}{dz^2} \cdot r\,dn.$$

In this expression we have

$$\frac{d\tau^2}{dz^2} = \frac{1}{v^2} = \frac{1}{1 + 2\varphi(\rho)}:$$

and as $2\varphi(\varrho)$, or the increment of the square of the velocity of the light is very minute, amounting to less than ·0006 in passing through the whole atmosphere to the earth's surface, we may reckon $\frac{d\tau^2}{dz^2}$ as unit; thus we get

$$d \,.\, \delta\theta = \frac{\phi(\rho')}{l} \cdot r\,dn;$$

and by integrating

$$\delta\theta = \frac{2\,\phi(\rho')}{\left(\frac{l}{a}\right)} \cdot \int \frac{r\,a\,dn}{2\,a^2}.$$

This result, which M. Biot has also obtained, is equivalent to the geometrical construction communicated by Newton to Flamsteed in a letter from Cambridge, December 20, 1694. The problem was now reduced to the quadrature of a curve, for which a general method is given in the fifth lemma of the third book of the Principia, a method which is still used when the direct process of integration fails, or becomes too intricate for practice. What has been said not only proves the exactness of Newton's solution of the problem; it also points out, with little uncertainty, the manner in which he obtained it. Of the arithmetical operations of the quadrature there is no account; and they would be of no interest had they been preserved. He complains much of the great labour of the numerical calculations; but all difficulties were overcome, as was to be expected: a table was computed and communicated to Flamsteed in a triple form, for summer, winter, and the intermediate seasons of spring and autumn. On mature reflection there occurred to him a serious objection to the supposed scale of densities, on which account he writes to Flamsteed that he does not intend to publish the tables. The fault lies in this, that the centripetal force which continually inflects the light to the earth's centre, is the same at all the points of the trajectory, or, in the words of Newton, the refractive power of the atmosphere is as great at the top as at the bottom,—than which nothing certainly can be more different from what actually takes place in nature.

Dismissing his first hypothesis, NEWTON next turned his attention to the 22nd proposition of the third book of his Principia. If the atmosphere consist of an elastic fluid gravitating to the earth's centre in the inverse proportion of the square of the distance, and if it be admitted that the densities are proportional to the pressures, NEWTON, in the proposition cited, proves in effect that the densities will form a decreasing geometrical series, when the altitudes are taken in arithmetical progression*. He writes to FLAMSTEED that an atmosphere so constituted is *certainly the truth.* NEWTON evidently intended by this assertion to mark a distinction between pressure, which is a cause of the variation of density that actually exists in nature, and his first assumed law of the densities, which is entirely arbitrary. Setting aside hypothesis, he now advanced so far in the true path of investigation; and if the manner in which heat is diffused in the atmosphere and the consequent decrease of density were not known when he wrote, he advanced as far as the existing state of knowledge enabled him to do. It is certain from his letters, that, after much time and labour, he at last succeeded in calculating a table of refractions on the principle that the density is proportional to the pressure. Such a table he communicated to FLAMSTEED, although it is not found in the letters lately published; and there is every reason to think it the same which he gave to HALLEY, and which that astronomer inserted in the Philosophical Transactions for 1721. Two elements only are sufficient for computing all the numbers in a table of refractions constructed by assuming that the density is proportional to the pressure, namely, the refraction at 45° of altitude, and the height of the homogeneous atmosphere, which is deducible from the horizontal refraction. The table of HALLEY, therefore, contains in itself all that is required for ascertaining whether it was calculated or not by the principle alluded to in the letters of NEWTON to FLAMSTEED. KRAMP seems to be the first who sought in the table for the manner of its construction; and his discoveries in this branch of science enabled him to assign the height of the homogeneous atmosphere, which is one essential element. The refraction at 45° of altitude, which is the other element, is found in the table equal to 54″, or, in parts of the radius, to ·0002618; and KRAMP found 4377† toises for l, the height of the homogeneous atmosphere; so that, if a be the radius of the earth in toises, we have

$$\alpha = \cdot 0002618,$$

$$i = \frac{l}{a} = \cdot 0013356;$$

and the two elements, α and i, are sufficient for computing the whole table, if it be

* NEWTON demonstrates strictly that the densities will be in geometrical proportion when the distances from the earth's centre are in musical or harmonical proportion, that is, when they are the reciprocals of an arithmetical progression; but in a series of this kind, if the first term bear an almost infinitely great proportion to the differences of the following terms, as is the case of the radius of the earth when compared to elevations within the limits of the atmosphere, the differences of the terms or the elevations may, without sensible error, be reckoned in arithmetical progression.

† Anal. des Réfractions Astronomiques, p. 19.

such as is mentioned in the correspondence between NEWTON and FLAMSTEED. The formula for the refraction in the supposed constitution of the atmosphere has been given both by KRAMP and LAPLACE; and it may be taken from the Paper in the Philosophical Transactions for 1823, p. 441,

$$\lambda = \frac{\alpha}{i} = \cdot 19601, \quad \Delta = \sqrt{\cos^2\theta + 2\,i\,s},$$

$$\begin{aligned}\delta\theta = \alpha\sin\theta \times \Big\{ & \left(1 - \lambda + \frac{\lambda^2}{2} - \frac{\lambda^2}{6}\right) . \int \frac{d\,s\,c^{-s}}{\Delta} \\ & + (\lambda - 2\,\lambda^2 + 2\,\lambda^3) . \int \frac{2\,d\,s\,c^{-2s}}{\Delta} \\ & + \left(\frac{3}{2}\,\lambda^2 - \frac{9}{2}\,\lambda^3\right) . \int \frac{3\,d\,s\,c^{-3s}}{\Delta} \\ & + \frac{8\,\lambda^3}{3} . \int \frac{4\,d\,s\,c^{-4s}}{\Delta},\end{aligned}$$

the integrations extending from $s = 0$ to $s = \infty$. The coefficients of this formula are as follows:

$$A = 1 - \lambda + \frac{\lambda^2}{2} - \frac{\lambda^3}{6} = \cdot 82193$$

$$B = \lambda - 2\,\lambda^2 + 2\,\lambda^3 = \cdot 13423$$

$$C = \frac{3}{2}\,\lambda^2 - \frac{9}{2}\,\lambda^3 \qquad = \cdot 02377.$$

$$D = \frac{8}{3}\,\lambda^3 \qquad = \cdot 02007.$$

For the horizontal refraction, when $\cos\theta = 0$, $\Delta = \sqrt{2\,i\,s}$, we obtain by the usual integrations,

$$\delta\theta = \frac{\alpha\,\sqrt{\varpi}}{\sqrt{2\,i}} \times \left\{A + B\sqrt{2} + C\sqrt{3} + D\sqrt{4}\right\}:$$

or in seconds, $\delta\theta = 2024{\cdot}2$ instead of $2025''$ as in HALLEY's table. This proves the exactness of KRAMP's elements.

With respect to the other numbers in the table a distinction must be made. In every table of refractions, whatever be the constitution of the atmosphere on which it is founded, the numbers answering to altitudes greater than 16°, depend only upon one element, namely, the refractive power of the air. Reckoning from the zenith as far as 74°, any table may be deduced from any other, provided both are accurately calculated, merely by a proportion. In the table published annually in the *Con. des Temps*, the refraction at 45° is $58''{\cdot}2$: and, if HALLEY's table has been accurately computed, the numbers in it, between the limits mentioned, will be equal to the like numbers in the French table multiplied by $\frac{540}{582} = \frac{90}{97}$. The calculation being made,

the results will be found to agree almost exactly with the short table inserted by M. Biot in the additions to the *Con. des Temps* for 1839, p. 105, the greatest difference between the computed quantities and the numbers in Halley's table being about 2″.

But this gives no intimation with respect to the particular constitution of the atmosphere assumed in the calculation of the table. What is peculiar to a table in this respect has no sensible influence on the refractions it contains except at altitudes less than 16°. No definitive opinion can therefore be formed on the question, whether Halley's table is the same which Newton computed and communicated to Flamsteed on the principle that the densities are proportional to the pressures, without comparing it with a sufficient number of refractions at low altitudes calculated from the elements of Kramp. As the settling of this point may be thought not unimportant, the following formula, which affords the means of computing the refractions at all altitudes with exactness, has been investigated by reducing the integrals in the expression of $\delta\,\theta$ to serieses.

$$\text{Tan}\,\varphi = \frac{2\sqrt{5i}}{\cos\theta}, \quad e = \tan\frac{\varphi}{2}.$$

$$\text{Log}\tan\varphi = \log\text{secant}\,\theta + 19{\cdot}2133569 - 20.$$

$$\delta\,\theta = \sin\theta \times \left\{\begin{array}{llll} & e \times & 660''{\cdot}795 \ldots\ldots & \overset{\text{log.}}{2{\cdot}8200669} \\ + & e^{3} \times & 551{\cdot}634 \ldots\ldots & 2{\cdot}7337059 \\ + & e^{5} \times & 371{\cdot}268 \ldots\ldots & 2{\cdot}5696873 \\ + & e^{7} \times & 219{\cdot}762 \ldots\ldots & 2{\cdot}3419630 \\ + & e^{9} \times & 116{\cdot}763 \ldots\ldots & 2{\cdot}0673034 \\ + & e^{11} \times & 58{\cdot}170 \ldots\ldots & 1{\cdot}7646976 \\ + & e^{13} \times & 28{\cdot}275 \ldots\ldots & 1{\cdot}4514092 \\ + & e^{15} \times & 13{\cdot}797 \ldots\ldots & 1{\cdot}1397974 \\ + & e^{17} \times & 6{\cdot}806 \ldots\ldots & 0{\cdot}8329041 \\ + & e^{19} \times & 3{\cdot}311 \ldots\ldots & 0{\cdot}5199046 \end{array}\right\}$$

The series converges very slowly, which has made it necessary to continue it to ten terms, the amount of which is still 3″·6 deficient from the exact quantity 2024″·2. As the last terms decrease in the proportion of 2 to 1, it is obvious that the true sum would be obtained by continuing the series: but the terms set down are more than sufficient for the present purpose.

The exact refractions calculated by the formula are next to be compared with the numbers in HALLEY's table.

Apparent zenith-dist.	Refractions		Difference.
	Computed.	HALLEY's Table.	
°	′ ″	′ ″	″
20	19·6	20	−0·4
40	45·2	45	+0·2
60	1 33·1	1 32	+1·1
70	2 27·0	2 26	+1·0
80	4 55·2	4 52	+3·2
82	6 3·9	6 00	+3·9
84	7 45·0	7 45	0·0
86	10 53·1	10 48	+5·1
87	13 22·5	13 20	+2·5
88	17 4·6	17 8	−3·4
89	23 3·5	23 7	−3·5

The examination that has now been made fully establishes that HALLEY's table is no other than the one which NEWTON computed on the supposition that the densities in the atmosphere are proportional to the pressures. The discrepancies, amounting in every instance to a small part of the whole quantity, are such as might be expected to occur unavoidably in the intricate and laborious methods of calculation followed by NEWTON. Some part of the differences may also arise from the elements of the formula, which, being deduced from only two numbers of the table, may not exactly coincide with the fundamental quantities which NEWTON assumed. M. BIOT, by a method different from KRAMP's, has found other elements, which make the horizontal refraction 8″·3 less than in the table; but these small variations, which proceed from calculating in different ways, only confirm more strongly that HALLEY's table is the same which NEWTON constructed by his second hypothesis, and communicated to FLAMSTEED.

It appears from what has been said, that, as far as the mathematics is concerned, the problem of the astronomical refractions was fully mastered by NEWTON. It would be strange indeed to suppose that the author of the theories in the Principia would find difficulty to apply them in a case to which he bent the whole force of his mind. But he was embarrassed by the suppositions he found it necessary to make respecting the physical constitution of the atmosphere. His first hypothesis is evidently contrary to nature, in admitting that air at all altitudes exerts the same power to inflect the light to the earth's centre; his second method made the refractions too great near the horizon, thereby proving the necessity of searching out some new cause for the purpose of reconciling the theory with observation. Averse from hypotheses, he seems, on these accounts, to have declined inserting in his works a problem which had cost him so much labour, and upon his solution of which he evidently set some value.

If the different attempts to solve this problem are to be tried with the same rigour

that NEWTON judged his own, it must be decided that they are all liable to objections. They all involve some supposition that has no foundation in nature; or they leave out some necessary condition of the problem. It is allowed that the variation of heat at different altitudes is unknown; that we are equally unacquainted with the manner in which is diffused the aqueous vapour that is always found, more or less, in the atmosphere; that the law of the densities has not been ascertained. But besides these capital points, the accurate M. POISSON will suggest other properties that must be attended to in an atmosphere in equilibrium: the conducting power of heat varying with the condition of the air; its power of absorbing heat; and the interchange by radiation which takes place with the earth, with the etherial spaces above, and with the stars visible above the horizon. So many conditions, placed beyond the reach of our inquiry, may well puzzle the most expert algebraist to take them into account. But it may be doubted whether this be the proper view of the problem. The astronomical refractions at any observatory are mean effects of the atmosphere; and it may be alleged that the proper way of accounting for them is to compare them with other mean effects of the atmosphere at the same place.

4. In 1715, twenty years after the researches of NEWTON, BROOK TAYLOR published in his *Methodus Incrementorum*, the first investigation of the astronomical refractions on the supposition that the density of the air is variable. The differential equation is accurately deduced from the principles laid down by NEWTON; which removed all the difficulties of the problem in this respect.

KRAMP, in his *Analyse des Réfractions Astronomiques et Terrestres*, has elucidated the elementary parts of the problem, and has greatly improved the mathematical solution. His method is particularly commodious and useful in the case of the horizontal refraction. For altitudes above the horizon the integrals are not susceptible of being simply expressed, and seem to require the aid of subsidiary tables in applying them.

The problem of the refractions being an important one in astronomy, many solutions of it have been published by different geometers. Some of these are preferable to others, because the method of calculation is easier in practice. For altitudes greater than 16°, they may all be reckoned equivalent, differing from one another only in the quantity assumed for the refractive power of air. They also mostly agree in the horizontal refraction, which is taken from observation. But for altitudes less than 16°, they are different: because, at these low altitudes, the refractions are affected by the arbitrary suppositions used in constructing the tables.

THOMAS SIMPSON published a judicious dissertation on this problem. He distinctly points out that, to a considerable distance from the zenith, the refractions are independent of the manner in which the atmosphere is supposed to be constituted. In comparing the two atmospheres that have densities decreasing in arithmetical and geometrical progression, he remarks that the horizontal refraction comes much nearer the observed quantity in the first atmosphere than it does in the second; for which

reason he gives the preference to the first as likely to represent the phenomena with greater accuracy. Now in this his reasoning is not much different from the argument afterwards used by LAPLACE, to prove that the same two atmospheres are limits between which the true atmosphere is contained.

NEWTON likewise found that the refractions computed according to his second method, that is, in an atmosphere with densities decreasing in geometrical progression, are too great near the horizon, on which point he thus writes to FLAMSTEED. "Supposing the atmosphere to be constituted in the manner described in the 22nd proposition of my second book (which certainly is the truth), I have found that if the horizontal refraction be 34′, the refraction at the altitude of 3° will be 13′ 3″; and if the refraction in the apparent altitude of 3° be 14′, the horizontal refraction will be something more than 37′. So that instead of increasing the horizontal refraction by vapours, we must find some other cause to decrease it. And I cannot think of any other cause besides the rarefaction of the lower region by heat." Here the true reason is assigned why the refractions near the horizon, in an atmosphere constituted as supposed, so much exceed the observed quantities. When the density is made to depend solely on the incumbent weight, the air is not rarefied enough; and the greater density causes a greater refraction. Having correctly estimated the effect produced by the pressure of the supported air, NEWTON is unavoidably led to ascribe to heat the greater rarefaction that takes place in the atmosphere of nature. His words prove that he had no clear conception in what manner the density in the lower region is altered by the agency of heat; and, to say the truth, nearly the same ignorance in this respect prevails now as in his time. The decrease of density in ascending is a complicated effect of many causes for the most part unknown; and it seems in vain to expect a satisfactory investigation of it by arbitrary suppositions. But setting aside hypothetical constitutions of the atmosphere, we may consider the rarefaction of the air in ascending as a phenomenon, the knowledge of which is to be acquired by experiment; and this appears the only sure way of placing the theory of the mean refractions on its proper foundation.

5. One of the tables of refraction most esteemed by astronomers is that published annually in the *Con. des Temps*. It has been already shown that, as far as 74° from the zenith, this table is calculated by the simple method of CASSINI. There is nothing incidental in this; for all tables of refraction may be computed by CASSINI's method to the extent mentioned. The French astronomers have been very successful in determining the constants of the formula. The refractive power of the air was obtained by DELAMBRE from a great number of astronomical observations; the same quantity was deduced by MM. BIOT and ARAGO from experiments on the gases with the prism; and the results of two methods, so entirely different, agree so nearly, that there seems no ground for preferring one to the other. The remaining part of the French table, for altitudes less than 16°, is computed by a method of LAPLACE, which the author has explained, without disguising its defects, in the fourth volume of the *Mec. Celeste*.

The two atmospheres with densities decreasing in arithmetical and geometrical progression, which it now appears were imagined by NEWTON, and which have been discussed by THOMAS SIMPSON and other geometers, are found, when the same elements are employed, to bring out horizontal refractions on opposite sides of the observed quantity. LAPLACE conjectured that an intermediate atmosphere which should partake of the nature of both, and should agree with observation in the horizontal refraction, would approach nearly to the true atmosphere. It must be allowed that these conditions, which may be verified by innumerable instances between the two limits, are vaguely defined; and in order to ascertain the real meaning of the author, recourse must be had to the algebraic expressions. When this is done it will be found that the atmosphere intended is one of which the density is the product of two terms, one taken from an arithmetical, and the other from a geometrical progression; the effect of which combination is to introduce a supernumerary constant, by means of which the horizontal refraction is made to agree with the true quantity. No one will deny the merit and the ingenuity of LAPLACE's procedure; but though very skilful, and guided in some degree by fact, it is liable to all the uncertainty of other arbitrary suppositions, as indeed the author allows. Dr. BRINKLEY has given the character of the French table fairly when he says, that it is only a little less empirical than the other tables. On divesting LAPLACE's hypothesis of vagueness in the language, and expressing it in the unequivocal symbols of algebra, it does not appear to possess any superiority over other supposed constitutions of the atmosphere in leading to a better and less exceptionable theory; at least the *Mec. Celeste* has been many years before the public, during which time not a few geometers have laboured on the subject of the refractions; but no improvement originating in the speculations peculiar to LAPLACE has occurred to any of them.

Having said so much on the theory of the French table, it may be proper to add a word on its accuracy. If it be compared from 80° to 88° of zenith distance with BESSEL's observed refractions, there will be found a small error in excess, continually increasing, and amounting at last to 4″. This shows that, in combining the two atmospheres, too much weight has been given to that with a density varying in geometrical progression, in consequence of which the air is not rarefied enough in the interpolated atmosphere. With respect to the two degrees of altitude next the horizon, no accurate judgement can be formed, for want of observed refractions that can be depended on.

The astronomical refractions have also occupied the attention of the astronomer of Kœnigsberg, who has contributed so largely to the improvement of every part of astronomical science. For the purpose of representing the observations of BRADLEY with all the accuracy possible, BESSEL investigated a table of refractions which appeared in the *Fundamenta Astronomiæ* in 1818. He assumes a theoretical formula; but as every arbitrary quantity is determined by a careful comparison with real observations, what is supposititious may be considered merely as an instrument of investigation, which is finally laid aside, leaving the result to rest on the foundation of

fact. He returned to the subject in his *Tabulæ Regiomontanæ*, published in 1830. In this last work he retains only that part of the table of 1818 which extends to 85° from the zenith, many corrections being applied from recent observations made with improved instruments. In order to supply what is wanting in the new table, Bessel has added a supplemental one containing the refractions at every half degree for altitudes less than 15°: which supplemental table is independent of theory, being deduced from observations alone. These two tables form together a real table of mean refractions, independent of all suppositions respecting the constitution of the atmosphere; and no other similar table of nearly equal authority is to be found in the astronomy of the present day. What Bessel has accomplished on the subject of the refractions is not the least important part of his labours for the advancement of astronomical science: it is precious to the practical astronomer; and it is necessary to the theoretical inquirer, for enabling him to confront his speculations with the phenomena to be accounted for.

6. In the paper published in the Philosophical Transactions for 1823, the refractions are deduced entirely from this very simple formula,

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1 - f(1 - c^{-u}), \quad \ldots\ldots\ldots\ldots \quad (4.)$$

in which β stands for the dilatation of air, or a gas, by heat; τ' is the temperature at the earth's surface, and τ the temperature at any height above the earth's surface; at the same height c^{-u} is the density of the air in parts of its density at the surface.

In order to understand the application of the formula, it is necessary to premise that in the remaining part of this paper we do not consider a variable atmosphere subject to continual fluctuations, as is the case of the real atmosphere: we contemplate an atmosphere fixed in its condition at any given place or observatory, being supposed a mean between all the variations that actually take place in an indefinite time. In such an atmosphere the temperature and pressure at the earth's surface will be mean quantities deduced from observation: the air at all elevations will have an elastic force equal to the incumbent weight which it supports, as an equilibrium requires: and, whether the air be dry or moist, its refractive power will be equal to the refractive power of dry air subjected to the same pressure and temperature*. These properties of the mean atmosphere rest upon experiment and demonstration: in other respects its nature is not directly known to us: and the laws of its action can only be discovered, not by hypothesis, but by observation.

The consideration of a mean atmosphere, invariable at any given observatory, is a necessary consequence of the notion we attach to the mean refractions; for these would be realized in such an atmosphere: but they are different in any other state of the air.

These observations being premised, if the formula (4.) be verified at the earth's

* Additions à la Conn. des Temps, 1839, p. 36.

surface in any invariable atmosphere, by giving a proper value to the constant f, it will still hold, at least with a very small deviation from exactness, at a great elevation, probably at a greater elevation than has ever been reached by man. In order to prove this, let the arbitrary function $\varphi(u)$ be added, so as to complete the formula by rendering it perfectly exact: then

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1 - f(1 - c^{-u}) - \varphi(u), \quad \text{(5.)}$$

and it will follow that $\varphi(u) = 0$, when $u = 0$, that is, at the earth's surface. Again, differentiate the equation, observing that τ decreases when u increases, then

$$\frac{\beta}{1+\beta\tau'} \cdot \frac{d\tau}{du} = f c^{-u} + \frac{d.\varphi(u)}{du}:$$

now, since this equation is true for all values of u, it will hold at the earth's surface, or when $u = 0$: and if f be taken equal to the particular value of

$$\frac{\beta}{1+\beta\tau'} \cdot \frac{du}{d\tau},$$

when $u = 0$, it will follow that $\frac{d.\varphi(u)}{du} = 0$, when $u = 0$. And since the equations $\varphi(u) = 0$ and $\frac{d.\varphi(u)}{du} = 0$, are both verified at the earth's surface, it follows that the supplementary function $\varphi(u)$ will vary slowly as u increases, that is, as the density of the air decreases in ascending. This proves that the approximate equation (4) will be very little different from the exact equation (5.) at great elevations in the atmosphere.

At the surface of the earth du is equal to the variation of the density for a depression of the thermometer expressed by $d\tau$: and although the proportion of these two quantities cannot be ascertained by direct experiment, yet, as is shown in the paper of 1823, it is easily deduced from the rate at which the temperature decreases as the height increases, which rate is easily determined experimentally. The quantity f thus found is necessarily constant at the same observatory. It is the mean of all the occasional values, which vary incessantly, while the real atmosphere undergoes every vicissitude of which it is susceptible. The mean refraction and f are invariable in quantity, because they depend alike upon the mean condition of the air at a given place. Some confusion has arisen on this point from not distinguishing between the mean refraction of a star and its true refraction in a variable atmosphere.

In all that has been said there is no supposition of an arbitrary constitution of the atmosphere. The assumed formula (4.) is an approximate truth in every invariable state of the air in equilibrium. Conceive a cylindrical column of air perpendicular to the earth's surface, and extending to the top of the atmosphere; at the height where the temperature is τ, and the density ϱ, let p denote the weight of the column above the height; and suppose that p, ϱ, τ are changed into p', ϱ', τ' at the surface of the

earth; because the pressures are proportional to the elasticities, we have the usual equation,

$$\frac{p}{p'} = \frac{1+\beta\tau}{1+\beta\tau'} \times \frac{\rho}{\rho'}:$$

or, which is the same,

$$c^{-u} = \frac{\rho}{\rho'}, \quad \frac{p}{p'} = \frac{1+\beta\tau}{1+\beta\tau'} \times c^{-u}:$$

and by substituting the complete expression of the temperature as given in (5.), we shall obtain,

$$\frac{p}{p'} = c^{-u} - f(c^{-u} - c^{-2u}) - c^{-u} \times \phi(u): \quad . \quad . \quad . \quad . \quad (6.)$$

and if we omit the supplemental part, which is small even at great elevations, the result will be,

$$\frac{p}{p'} = c^{-u} - f(c^{-u} - c^{-2u}).$$

Now this is the constitution of the atmosphere in the paper of 1823; it is only approximate; but it is an approximation applicable to every atmosphere that can be imagined, requiring no more than that the quantity f have the proper experimental value given to it. It is shown in the paper that the pressures and densities are thus represented with no small degree of accuracy at the greatest heights that have been reached; which proves how slowly the supplemental part of the formula (5.) comes into play.

The foregoing manner of arriving at the constitution of the atmosphere adopted in the paper of 1823, being drawn from properties immediately applicable to the problem in hand, is more natural, and more likely to suggest itself, and more satisfactory than the ingenious and far-fetched procedure of M. Biot, of transforming an algebraic formula for the express purpose of bringing out a given result. Laplace, having remarked that the true horizontal refraction is contained between the like quantities of two atmospheres, with densities decreasing in arithmetical and geometrical progression, conjectured that an atmosphere between the two limits, which should likewise agree with observation at the horizon, would in all probability represent the mean refractions with considerable accuracy. It is upon this assumption that the problem is solved in the *Mec. Celeste*, the observed horizontal refraction being used for determining the arbitrary constant. Now in the paper of 1823 there is no allusion to interpolating an atmosphere between two others; a knowledge of the horizontal refraction is not required; the investigation is grounded upon a property common to every atmosphere in a quiescent state; and lastly, the resulting table is essentially different from all the tables computed by other methods. If these considerations be not sufficient to stamp an appropriate character upon the solution of a problem, it would be difficult to find out what will be sufficient. But if it be possible, with M. Biot's ingenuity, to trace some relation in respect of the algebraic

expressions, between the paper of 1823 and the calculations of LAPLACE, from which, after all, no just inference can be drawn, it is not difficult to find between the same paper and the view of the problem taken by the author of the Principia, in 1696, an anology much more simple and striking, which deserves to be mentioned as it tends to bring back the investigation to the right tract, which it seems to have left. NEWTON, having solved the problem on the supposition that the density of the air is produced solely by pressure, found that the refractions thus obtained greatly exceeded the observed quantities near the horizon: and hence he inferred, in the true spirit of research, that there must be some cause not taken into account, such as the agency of heat, which should produce, in the lower part of the atmosphere, the proper degree of rarefaction necessary to reconcile the theoretical with the observed refractions. Now, in the paper of 1823, the sole intention of introducing the quantity f, not noticed before by any geometer, is to cause the heat at the earth's surface to decrease in ascending at the same rate that actually prevails in nature; which evidently has the effect of supplying the desideratum of NEWTON.

The remarks that have just been made are not called for by anything which M. BIOT has written in his dissertation on the refractions, inserted in the additions to the *Conn. des Temps* for 1839; because that author has fully explained the grounds of what he advances, thereby enabling a candid inquirer to form his own opinion: but all the world are not of the same character as that distinguished philosopher.

At every point on the earth's surface we are now acquainted with three things, not hypothetical or precarious, that have an influence on the mean refractions. These are, the refractive power of the air, the spherical figure of the atmosphere, and the mean rate at which the density of the air decreases at the given place. These three things are independent on one another, and on all other properties of the air: they will therefore produce three independent parts of the quantity sought. The parts thus determined may fall short of the whole refraction at any altitude, because there may be causes not taken into account that co-operate in producing the result: but each will unalterably maintain its proper share of the total amount, in whatever way it is attempted to solve the problem, provided the solution is conducted on right principles and not warped by arbitrary suppositions. It may therefore be said that, in so far, an advance has been made in acquiring an exact notion of the nature of this problem.

The table in the paper of 1823 was compared with the best observations that could be procured at the time of publication; and the results were very satisfactory. After the publication of the *Tabulæ Regiomontanæ*, it was found that the table agreed with BESSEL's observed refractions to the distance of 88° from the zenith, which is as far as his determinations can be depended on, with such small discrepancies as may be supposed to exist in the observations themselves. So close an agreement between the theoretical and observed mean refractions was very unexpected, and even contrary to the opinion very generally held on this subject.

Astronomers are in the habit of using different tables or formulas of refraction, which, being derived from conjectural views, do not agree with one another, except to a limited distance from the zenith. Now this is contrary to the very conception we have of the mean refractions, which are determinate and invariable numbers, at least at the same observatory. A great advantage would therefore ensue from setting aside every uncertain table, and substituting in its place one deduced from the causes really existing in nature that produce the phenomena. Such a table adapted to every observatory, if this were found necessary, would contribute to the advancement of astronomy by rendering the observations made at different places more accurately comparable. It might contribute to the advancement of knowledge in another respect: for if the mean refractions were accurately settled, the uncertainty in the place of a star would fall upon the occasional corrections depending on the indications of the meteorological instruments; and it is not unreasonable to expect that much which is at present obscure and perplexing on this head might be cleared up, if it were separated from all foreign irregularities, and made the subject of the undivided attention of observers.

7. The paper in the Philosophical Transactions for 1823 takes into account only the rate at which the densities in a mean atmosphere vary at the surface of the earth; what follows is an attempt to complete the solution of the problem by estimating the effect of all the quantities on which the density at any height depends. For this purpose it will be requisite to employ certain functions of a particular kind, viz.

$$\mathrm{R}_1 = 1 - c^{-u},$$

$$\mathrm{R}_2 = 1 - u - c^{-u},$$

$$\mathrm{R}_3 = 1 - u + \frac{u^2}{1.2} - c^{-u},$$

$$\vdots$$

$$\mathrm{R}_i = \left(1 - u + \frac{u^2}{1.2} - \frac{u^3}{1.2.3} \cdots \pm \frac{u^{i-1}}{1.2.3\ldots i-1}\right) - c^{-u}.$$

In these expressions c is the number of which the hyperbolic logarithm is unit; and it is obvious that R_i is zero when $u = 0$. These expressions have several remarkable properties, which are proved by merely performing the operations indicated.

1st. $$\frac{d.\mathrm{R}_i}{du} = -\mathrm{R}_{i-1},$$

$$\int -\mathrm{R}_i\, du = \mathrm{R}_{i+1},$$

the integral being taken equal to zero, when $u = 0$.

2ndly. $$\frac{d.c^{-u}\mathrm{R}_i}{c^{-u}.du} = -(\mathrm{R}_{i-1} + \mathrm{R}_i),$$

$$\frac{d\,d\,.\,c^{-u}\,\mathrm{R}_i}{c^{-u}\,.\,d\,u^2} = \mathrm{R}_{i-2} + 2\,\mathrm{R}_{i-1} + \mathrm{R}_i$$

$$\frac{d^3\,.\,c^{-u}\,\mathrm{R}_i}{c^{-u}\,d\,u^3} = -\,(\mathrm{R}_{i-3} + 3\,\mathrm{R}_{i-2} + 3\,\mathrm{R}_{i-1} + \mathrm{R}_i),$$

&c.

3rdly. n being less than i,

$$\int \frac{d^n\,.\,c^{-u}\,\mathrm{R}_i}{c^{-u}\,.\,d\,u^n}\,d\,u = (-\,1)^n \cdot \frac{d^n\,.\,c^{-u}\,\mathrm{R}_{i+1}}{c^{-u}\,.\,d\,u^n}.$$

These things being premised, the temperature of an atmosphere in equilibrium will have for its complete expression this formula,

$$\frac{1+\beta\,\tau}{1+\beta\,\tau'} = 1 - f\,\mathrm{R}_1 - f' \frac{d\,.\,c^{-u}\,\mathrm{R}_3}{c^{-u}\,.\,d\,u} - f'' \cdot \frac{d^2\,.\,c^{-u}\,\mathrm{R}_5}{c^{-u}\,.\,d\,u^2} - \text{\&c.} \quad . \quad . \quad . \quad . \quad . \quad . \quad (7.)$$

the coefficients f, f', f'', &c. being indeterminate constant quantities. A little attention will show that this expression is equivalent to a series of the powers of u; for, first, let the differential operations in the several terms be performed, which will bring out

$$\frac{1+\beta\,\tau}{1+\beta\,\tau'} = 1 - f\,\mathrm{R}_1 + f'\,(\mathrm{R}_2 + \mathrm{R}_3) - f''\,(\mathrm{R}_3 + 2\,\mathrm{R}_4 + \mathrm{R}_5) + \text{\&c.};$$

next, expand R_1, R_2, &c., and the result will be,

$$\begin{aligned}\frac{1+\beta\,\tau}{1+\beta\,\tau'} = 1 - f\,u &+ (f - f') \cdot \frac{u^2}{1\,.\,2} \\ &- (f - 2f' + f'') \cdot \frac{u^3}{1\,.\,2\,.\,3} \\ &+ (f - 2f' + 3f'' - f''') \frac{u^4}{1\,.\,2\,.\,3\,.\,4} \\ &- \text{\&c.}\end{aligned}$$

The intention of assuming the formula (7.) is to express the temperature in terms of such a form as will produce, in the refraction, independent parts that decrease rapidly.

In order to elucidate what is said, and more especially to prove that the analysis here followed comprehends all atmospheres, whether of dry air or of air mixed with aqueous vapour; let p', ϱ', τ' denote, as before, the pressure, the density, and the temperature, at the surface of the earth; and put p, ϱ, τ for the like quantities at the elevation z above the surface: the equations of equilibrium are these two, the radius of the earth being represented by a, viz.

$$p = \int \frac{-\,d\,z\,.\,\varrho}{\left(1 + \frac{z}{a}\right)^2},$$

$$\frac{p}{p'} = \frac{1+\beta\,\tau}{1+\beta\,\tau'} \cdot \frac{\varrho}{\varrho'}.$$

The second of these equations has already been noticed: the integral in the first being extended to the top of the atmosphere, is equal to the weight of the column of air above the initial height, every infinitesimal mass being urged by a gravitation which is equal to unit at the earth's surface, and decreases in the inverse proportion of the square of the distance from the earth's centre. By putting

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1-q, \quad \sigma = \frac{z}{1+\frac{z}{a}}, \quad \frac{\varrho}{\varrho'} = c^{-u},$$

the same two equations will be thus written, viz.

$$p = \varrho' \int - d\sigma\, c^{-u},$$

$$p = p'(1-q)\, c^{-u}.$$

The three quantities u, q, σ, are severally equal to zero at the earth's surface: and the two values of p will not be identical, unless the same three quantities can be expressed by functions of one variable, or, which is equivalent, unless two of them, as q and σ, are each functions of the remaining one u. Now q being a function of u, we shall have,

$$q = \frac{dq}{du}\cdot u + \frac{ddq}{du^2}\cdot\frac{u^2}{1.2} + \frac{d^3q}{du^3}\cdot\frac{u^3}{1.2.3}\ \&c.,$$

the differentials being valued when $u = 0$, that is, the particular values which they have at the earth's surface being taken. According to what was before shown, we have this other series for q, viz.

$$q = fu - (f-f')\cdot\frac{u^2}{1.2} + (f-2f'+f'')\cdot\frac{u^3}{1.2.3}\ \&c.:$$

and as the two series must be identical, it follows that the quantities f, f', f'', &c., will be known, if we can ascertain the particular values assumed at the surface of the earth by the differentials of q considered as varying with u, or with the density. Thus the coefficients in the formula (7.) are not hypothetical quantities, but such as have a real existence in nature, and which might be determined experimentally, if we had the means of observing the phenomena of the atmosphere with sufficient exactness, so as to be able to determine q when u is given. It is further to be observed, that the same formula is general for all atmospheres, whether the air be entirely dry, or mixed with aqueous vapour: for it has been investigated from equations common to all atmospheres in equilibrium, without any consideration of a particular state of the air.

By substituting the series for q in the equation

$$\frac{p}{p'} = (1-q)\, c^{-u},$$

we obtain,

$$\frac{p}{p'} = c^{-u} - f c^{-u} R_1 - f' \frac{d.c^{-u} R_3}{du} - f''.\frac{d^2.c^{-u} R_5}{du^2} - \&c. \quad . \quad . \quad . \quad . \quad (8.)$$

Further, if this value of $\frac{p}{p'}$ be substituted in the equation

$$\frac{p}{p'} = \frac{\varrho'}{p'} \int - d\sigma\, c^{-u},$$

we shall find

$$\int - d\sigma\, c^{-u} = \frac{p'}{\varrho'} \cdot \left\{ c^{-u} - f \,.\, c^{-u} \mathrm{R}_1 - f' \frac{d \,.\, c^{-u} \mathrm{R}_3}{du} - \&c. \right\}.$$

Now, let this expression be differentiated; then divided by c^{-u}; and, finally, integrated, attending to the nature of the functions concerned; and the following result will be obtained:

$$\sigma = \frac{z}{1 + \frac{z}{a}} = \frac{p'}{\varrho'} \left(u - f \cdot \frac{d \,.\, c^{-u} \mathrm{R}_2}{c^{-u} \,.\, du} - f' \frac{d\, d \,.\, c^{-u} \mathrm{R}_4}{c^{-u} \,.\, du_2} - \&c. \right) \quad . \quad . \quad . \quad . \quad . \quad . \quad (9.)$$

The equations (7.), (8.), (9.) contain the theoretical explanation of the properties of the atmosphere. What is said may easily be proved by applying them to such phenomena as have been ascertained in a satisfactory manner. This application is besides necessary for determining the numerical values of the coefficients f, f', f'', &c., which enter into the expression of the refraction. For this purpose it is requisite to find the relations that subsist between the pressure, the temperature, and the height above the earth's surface, by combining the equations so as to exterminate u.

By performing the differentiations in the equation (9.), there will be obtained,

$$\sigma = \frac{p}{\rho} \{ u + f(\mathrm{R}_1 + \mathrm{R}_2) - f'(\mathrm{R}_2 + 2\,\mathrm{R}_3 + \mathrm{R}_4) + \&c.:$$

and, by expanding the functions,

$$\sigma = \frac{p'}{\rho'} \cdot \left\{ (1 + f)\, u - (2f - f') \cdot \frac{u^2}{2} + (2f - 3f' + f'') \frac{u^3}{1.2.3} - \&c. \right\}.$$

Now, by reverting the series for q, we get

$$u = \frac{q}{f} + \frac{f - f'}{2f} \cdot \frac{q^2}{f^2} + \frac{2f^2 - 4ff' + 3f'^2 - ff''}{6f^2} \cdot \frac{q^3}{f^3} + \&c.;$$

and, by substituting this value of u, the following formula will be obtained:

$$\sigma = \frac{z}{1 + \frac{z}{a}} = \frac{p'}{\rho'} \cdot \left(\frac{1 + f}{f} \cdot q + \frac{f - f' - f^2}{2f^3} \cdot q^2 + \&c. \right) . \quad . \quad . \quad . \quad . \quad . \quad (A.)$$

This equation between the perpendicular elevation z, and the difference of temperature

$$q = \frac{\beta(\tau' - \tau)}{1 + \beta\tau'},$$

contains the law according to which the heat decreases as the height above the earth's surface increases.

Further, from the equation

$$\frac{p}{p'} = (1 - q)\, c^{-u},$$

we deduce

$$\log\left(\frac{p'}{p}\right) = u + \log\frac{1}{1-q} = u + q + \frac{q^2}{2} + \frac{q^3}{3} + \&c.;$$

and, by substituting the value of q,

$$\log\frac{p'}{p} = (1+f)\,u - \frac{f - f' - f^2}{2}\,u^2 + \frac{f - 2f' + f'' - 3f^2 + 3ff' + 2f^3}{6}\,u^3, \&c.$$

By means of this series and the value of σ in terms of u already found, it is easy to deduce

$$\sigma = \log\left(\frac{p'}{p}\right) \times \frac{p'}{\varrho'}\left(1 - \frac{f}{2}\cdot u + \frac{2f - 2f' + 3f^2 - 3ff' - f^3}{12(1+f)}\cdot u^2 - \&c.\right);$$

and, by substituting the value of u, we finally obtain

$$\sigma = \frac{z}{1 + \frac{z}{a}} = \log\frac{p'}{p} \times \frac{p'}{\rho'}\cdot\left(1 - \frac{1}{2}\,q - \frac{f + f^3 - f'}{12 f^2 (1+f)}\cdot q^2, \&c.\right). \quad . \quad . \quad \text{(B.)}$$

This formula determines a perpendicular ascent z, when the difference of the pressures, and of the temperatures, at its upper and lower extremities, have been found.

The formulas that have been investigated are true in an atmosphere of air mixed with aqueous vapour, as well as in one of perfectly dry air; but in applying them, perspicuity requires that the two cases be separately considered.

Atmosphere of dry air.

In applying the formula (A.) to the experimental ascents that have been made in the atmosphere, σ may be accounted equal to z, the height ascended: for $\frac{z}{a}$, which is a minute fraction at the top of the atmosphere, is insensible in small elevations. Further, in such experiments, the depression of the thermometer, or the difference of the temperature at the upper and lower extremities of the ascent, is only a moderate number of degrees; and as β is a very small fraction, the value of q in the formula

$$q = \frac{\beta(\tau' - \tau)}{1 + \beta\tau'},$$

will be so inconsiderable, that its powers may be neglected. Attending to what is said, the formula (A.), even in those cases where the ascents are most considerable, may take this very simple form without much error, or rather with all the accuracy warranted by the nature of such experiments, viz.

$$z = \frac{p'}{\rho'}\cdot\frac{1+f}{f}\cdot\frac{\beta(\tau' - \tau)}{1 + \beta\tau'};$$

or, by making $D = \varrho'(1 + \beta\tau')$,

$$z = \frac{1+f}{f}\cdot(\tau' - \tau)\,.\,\beta\cdot\frac{p'}{D}.$$

Now it is obvious that D is the density of the air at the earth's surface, reduced to zero of the thermometer; and hence we learn that $\frac{p'}{D}$ is independent on the magnitude of p', and has the same value in all atmospheres of dry air; for, D being the density of the air produced by the pressure p' at the fixed temperature zero of the thermometer, it will vary proportionally to p'.

The value of the constant quantity $\frac{p'}{D}$ is next to be found. It has been ascertained, by very careful experiments, that the density of mercury is to the density of dry air as 10462 to 1, the temperature being 0° centigrade, or 32° of FAHRENHEIT's scale, and the barometric pressure $0^{m}{\cdot}76$, or 29·9218 English inches. The temperature remaining at 32° FAHRHENHEIT, if the barometric pressure be changed to p', the density of mercury will be to the density of dry air, at the temperature 32° FAHRHENHEIT and under the pressure p', as $10462 \times \frac{29{\cdot}9218}{p'}$ to 1; wherefore, as D stands for the density of dry air in the circumstances mentioned, its value estimated in parts of the density of mercury, will be thus expressed:

$$D = \frac{1}{10462} \times \frac{p'}{29{\cdot}9218}:$$

hence

$$\frac{p'}{D} = 10462 \times 29{\cdot}9218;$$

and, by reducing the inches to fathoms,

$$\frac{p'}{D} = L = 4347{\cdot}8 \text{ fath.}$$

This quantity being found, we deduce from the foregoing formula for z,

$$\frac{1+f}{f} = \frac{1}{\beta L} \times \frac{z}{\tau' - \tau}.$$

A single experiment in which z and $\tau' - \tau$ were ascertained, should be sufficient for determining $\frac{1+f}{f}$ and f: but it is well known that great irregularity prevails in the rate at which the heat decreases in the atmosphere, more especially when the elevations are small. This is owing chiefly to the thermometer, which is often affected by local and temporary causes. When we reflect that a considerable variation in the height is required to produce a small change of the thermometer, even the errors unavoidable in the use of that instrument must produce notable discrepancies in the rate, when the whole observed difference of temperature is only a few degrees. It thus appears that the quantity sought cannot be determined with tolerable exactness, except by taking a mean of the results obtained from many experiments. In this view, the average estimations of the decrease of heat in the atmosphere, which have been inferred from their own researches by philosophers on whose judgement and accuracy dependence can be had, becomes very valuable. Professor PLAYFAIR, in his

Outlines, states that the decrease of heat is nearly uniform for the greatest heights we can reach; and that it may be taken on an average as equal to 1° of FAHRENHEIT's thermometer for 270 feet, or 45 fathoms, of perpendicular ascent. The same rate has the authority of Professor LESLIE, to whom Meteorology is so much indebted. If we make $z = 45$ fathoms, $\tau' - \tau = 1°$, $\beta = \frac{1}{480}$, we shall obtain

$$\frac{1+f}{f} = \frac{45}{9{\cdot}05} = 5, f = \frac{1}{4},$$

which are the numbers assumed in the paper of 1823.

According to Dr. DALTON, another eminent philosopher who has studied meteorology very successfully, and made many experiments with great care, the average ascent for depressing FAHRENHEIT's thermometer 1° is 300 feet, or 50 fathoms: this gives

$$\frac{1+f}{f} = \frac{50}{9{\cdot}05} = 5{\cdot}5.$$

RAMOND, in his Treatise on the Barometrical Formula, has recorded the heights for depressing the centigrade thermometer 1°, in 42 different experiments. Setting aside four of this great number on account of their excessive irregularity, he states the mean of the remaining 38 at $164^{m}{\cdot}7$. A good average may be expected from so many experiments, made by observers of the greatest eminence, in different quarters of the world, in every variety of height and temperature. Now 4347·8 fathoms $= 7951^{m}$; $\beta = \frac{3}{800}$; $z = 164{\cdot}7$; consequently

$$\frac{1+f}{f} = \frac{800 \times 164{\cdot}7}{7951 \times 3} = 5{\cdot}5$$

It would be a great omission in this research to leave out the celebrated ascent of GAY LUSSAC in a balloon. Acccording to LAPLACE, the whole height ascended, or z, is 6980^{m}, the depression of the thermometer, or $\tau' - \tau$, being $40°{\cdot}25$ centigrade: hence

$$\frac{1+f}{f} = \frac{800}{7951 \times 3} \times \frac{6980}{40{\cdot}25} = 5{\cdot}8.$$

It is to be observed that, although experience and theory both concur in proving that z and $\tau' - \tau$ increase together in the same proportion to considerable elevations in the atmosphere, yet, at very great elevations, there is no doubt that z increases in a greater ratio than $\tau' - \tau$: so that when very great heights are used for computing $\frac{1+f}{f}$, the resulting value will be greater than the true quantity. What is said accounts sufficiently for the excess of $\frac{1+f}{f}$ deduced from GAY LUSSAC's ascent, above the other values found from moderate elevations. Without further research we may adopt the following determinations as near approximations derived from a multitude of experiments,

$$\frac{1+f}{f} = 5{\cdot}5\,; f = \frac{2}{9}.$$

The difference of these numbers from those used in the paper of 1823, produces an increase in the refractions, amounting to 19″ at the horizon, and to 2″ at 2° of altitude.

The irregular manner in which the heat decreases in such experiments as have been used for finding f, evidently makes them altogether unfit for determining the next coefficient f'. One remark respecting this quantity deserves to be noticed. By expanding the formula (4.), we obtain,

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1 - fu + f\cdot\frac{u^2}{1.2} - f\cdot\frac{u^3}{1.2.3} + \&c.:$$

the exact value is

$$\frac{1+\beta\tau}{1+\beta\tau'} = 1 - fu + (f - f')\cdot\frac{u^2}{1.2} - \&c.:$$

now, as these values continue very nearly equal to considerable elevations, the first terms of the two series must nearly coincide: which requires that f' shall be only a small part of f.

We have next to attend to the formula (B.). As q is only a small fraction in all the elevations that have been reached in the atmosphere, its square and other powers may be neglected: so that,

$$\sigma = \frac{z}{1+\frac{z}{a}} = \frac{p'}{\rho'}\left(1 - \frac{q}{2}\right)\log\left(\frac{p'}{p}\right):$$

and, because

$$1 - \frac{q}{2} = \frac{1+\beta\left(\frac{\tau'+\tau}{2}\right)}{1+\beta\tau'}; \quad \frac{p'}{\rho'(1+\beta\tau')} = \mathrm{L}:$$

$$z = \left(1+\frac{z}{a}\right).\mathrm{L}\left(1+\beta\left(\frac{\tau'+\tau}{2}\right)\right).\log\left(\frac{p'}{p}\right).$$

Now this is nothing more than the usual barometric formula for measuring heights, as it is found in the writings of LAPLACE or POISSON. It supposes that unit represents the force of gravity at the earth's surface; and if the variable intensity of that force in different latitudes must be taken into account, nothing more is requisite than to multiply by the proper factor. When this is done the foregoing expression will be identical with the usual formula, all its minutest corrections included. But there is this difference between the two cases, that the usual formula is investigated on the arbitrary supposition that the temperature is constant at all the points of an elevation, and equal to the mean of the temperatures at the two extremities; whereas the other expression is strictly deduced from the general properties of an atmosphere in equilibrium. The exact theoretical formula has been made to coincide with the approximate one, by dismissing all the terms that cannot be estimated in the present state of our knowledge of the phenomena of the atmosphere.

All the properties of the atmosphere that have been ascertained with any degree of certainty, have been made known to us by the application of the barometric formula:

it would therefore be superfluous to attempt, by the consideration of particular experiments, any further elucidation of a theory which is, in a manner, identical with observation, as far as our knowledge extends.

Atmosphere of air mixed with aqueous vapour.

Continuing to represent the pressure and temperature at the earth's surface by p' and τ', and the like quantities at the height z by p and τ, the symbols (ϱ'), (ϱ) may be used to denote the respective densities in the case of air mixed with aqueous vapour. When the pressure and density vary, all the gases, and mixtures of gases and vapours, are found to follow the same laws of dilatation and compression: and hence the same equations that express the equilibrium of an atmosphere of dry air, will hold equally in one of moist air. In the present case these equations will therefore be,

$$p = \int \frac{-dz.(\rho)}{\left(1+\frac{z}{a}\right)^2},$$

$$\frac{p}{p'} = \frac{1+\beta\tau}{1+\beta\tau'} \cdot \frac{(\rho)}{(\rho')}\text{*}:$$

and if we put

$$\sigma = \frac{z}{1+\frac{z}{a}}, \quad \frac{1+\beta\tau}{1+\beta\tau'} = 1-q, \quad \frac{(\rho)}{(\rho')} = c^{-u},$$

the same equations will be thus written,

$$p = (\varrho')\int -d\sigma\, c^{-u},$$

$$p = p'(1-q)\, c^{-u}.$$

The three quantities σ, q, u are severally equal to zero at the surface of the earth: so that, by the same procedure as before, we shall obtain these formulas,

$$q = fu - (f-f')\frac{u^2}{2} + \&c.$$

$$\sigma = \frac{p'}{(\rho')} \cdot \left\{u - f.\frac{d.c^{-u}R_2}{c^{-u}du} - f'.\frac{dd.c^{-u}R_2}{c^{-u}du^2} - \&c.\right.$$

But it is to be observed that, in these expressions, the coefficients f, f', &c., are not exactly the same as in an atmosphere of dry air: for the quantities mentioned, although they have determinate values in the same quiescent atmosphere, depend upon the manner in which the temperature q, or the height z, varies relatively to the density, or to u.

If we suppose that the height z is not very great, so that the powers of q may be

* This equation is equivalent to the one in p. 18 of M. Biot's dissertation, on which that author lays so much stress.

neglected, we shall obtain from the foregoing equations,

$$z = \frac{p'}{(\varrho')} \cdot \frac{1+f}{f} \cdot q:$$

and hence

$$\frac{1+f}{f} = \frac{1+\beta\tau'}{\beta} \cdot \frac{(\varrho')}{p'} \cdot \frac{z}{\tau'-\tau}.$$

In order to ascertain how far this value is different from the like value in the case of dry air, we must resolve the complex density (ϱ') into its elements. The hygrometer will discover the tension of the vapour at the earth's surface; and if φ' denote this tension in inches of mercury, and ϱ' be the density of dry air under the pressure p' and at the temperature τ', the following equation is proved in all the late treatises on Natural Philosophy,

$$(\varrho') = \varrho' \left(1 - \frac{3}{8} \cdot \frac{\varphi'}{p'}\right):$$

by means of which we obtain

$$\frac{1+f}{f} = \frac{1 - \frac{3}{8} \cdot \frac{\varphi'}{p'}}{\beta \mathrm{L}} \times \frac{z}{\tau'-\tau},$$

$$\mathrm{L} = \frac{p'}{\varrho'(1+\beta\tau')}.$$

Now the small additional factor in the value of $\frac{1+f}{f}$ is not taken into account in the measurement of heights by the barometer, no distinction being usually made between dry air and moist air. In order to form some estimate of its effect, we may instance the mean atmosphere of our climate, the temperature of which is 50° FAHRHENHEIT; the greatest possible tension of vapour in such an atmosphere is ·36 of an inch of mercury; at a medium, if we make $\varphi' = \cdot 18$, and $p' = 30$ inches, we shall have,

$$1 - \frac{3}{8} \cdot \frac{\varphi'}{p'} = 1 - \frac{1}{444}.$$

It thus appears that in our climate, when the mean portion of aqueous vapour is mixed with the air, the value of $\frac{1+f}{f}$ is less than it would be if the air were perfectly dry by its $\frac{1}{444}$th part, a quantity too minute to be perceptible in most experiments. A small part only of the refractions depend upon f, about a twelfth part of the whole at the horizon; so that, neglecting the minute variations which f undergoes by the greater or less portions of aqueous vapour mixed with the air, the effect of which on the refractions is insensible, we may assume that it has the same value in all atmospheres. The same thing applies with greater force to the other coefficients f', f'', &c., which having themselves hardly any influence on the refractions, their minute changes in different atmospheres may be wholly disregarded.

If we substitute for (ϱ') its equivalent $\varrho'\left(1 - \frac{3}{8} \cdot \frac{\varphi'}{p'}\right)$ in the foregoing value of σ,

we shall obtain the following equation, which is sufficient for the problem of the refractions in an atmosphere of moist air:

$$\sigma = \frac{1}{1 - \frac{3}{8} \cdot \frac{\phi'}{p'}} \cdot \frac{p'}{\rho'} \cdot \left\{ u - f \cdot \frac{d \cdot c^{-u} R_2}{c^{-u} d u} - f' \cdot \frac{d d \cdot c^{-u} R_4}{c^{-u} d u^2} - \&c. \right\} \ . \ . \ . \quad (10.)$$

In which expression the coefficients f, f', &c., may be considered the same in all atmospheres, the quantity u varying from zero at the earth's surface to be infinitely great at the top of the atmosphere.

8. In the foregoing analysis, every formula has been strictly deduced from the equations of equilibrium: no quantities have been introduced except such as really exist in nature, and might be determined experimentally, if we had the means of exploring the phenomena of the atmosphere with the requisite accuracy. It may not be improper to notice here an obvious consequence of the equation

$$p = \varrho' \int - d\sigma\, c^{-u},$$

which holds in an atmosphere of dry air; namely, that the integral

$$\int - d\sigma\, c^{-u},$$

being extended from the surface of the earth to the top of the atmosphere, is the analytical expression of $\frac{p'}{\varrho'}$, or of the height of the homogeneous atmosphere, that is, of a column of air equiponderant to the whole atmosphere, and every part of which has the same density and the same weight which it would have at the surface of the earth. This height varies only with the temperature, and is thus determined:

$$\frac{p'}{\rho'} = \frac{p'}{\rho'(1 + \beta \tau')} \cdot (1 + \beta \tau') = \frac{p'}{D} (1 + \beta \tau') = L (1 + \beta \tau').$$

In like manner, in an atmosphere of air mixed with aqueous vapour, the same integral is equal to $\frac{p'}{(\rho')}$: and we have

$$\frac{p'}{(\rho')} = \frac{p'}{\rho'} \cdot \frac{1}{1 - \frac{3}{8} \cdot \frac{\phi'}{p'}} = \frac{L (1 + \beta \tau')}{1 - \frac{3}{8} \cdot \frac{\phi'}{p'}}.$$

Thus the analytical theory agrees in every respect with the real properties of the atmosphere, as far as these have been ascertained; and we now proceed to show that the same theory represents the astronomical refractions with a fidelity that can be deemed imperfect only in so far as the constants f, f', &c., which can only be determined by experiment, are liable to the charge of inaccuracy.

9. The apparent zenith-distance of a star being represented by θ, and the refraction by $\delta\theta$, the following formulas have already been obtained (§ 2. equations (2.) and (3.)).

$$d \cdot \delta\theta = \frac{d y}{\sqrt{r^2 - y^2}},$$

$$y = a \sin \theta \times \sqrt{\frac{1 + 2 \phi (\rho')}{1 + 2 \phi (\rho)}},$$

the quantity $\delta \theta$ being supposed to increase from the surface of the earth to the top of the atmosphere. For the sake of perspicuity, we shall, in the first place, confine our attention to an atmosphere of dry air, in which case it is known by experiment that the refractive power $\varphi (\varrho)$ is proportional to the density ϱ; so that

$$\varphi (\varrho) = \mathrm{K} \times \varrho,$$

K being a constant. Adverting to the mode of expression before used, we have

$$\varrho = \varrho' c^{-u};$$

and hence

$$\varphi (\varrho) = \mathrm{K} \times \varrho = \mathrm{K} \varrho' . c^{-u},$$

$$y = a \sin \theta \times \sqrt{\frac{1 + 2 \mathrm{K} \rho'}{1 + 2 \mathrm{K} \rho' c^{-u}}}:$$

and by introducing new symbols in order to abridge expressions,

$$\alpha = \frac{\mathrm{K} \rho'}{1 + 2 \mathrm{K} \rho'},$$

$$\omega = 1 - c^{-u},$$

$$y = \frac{a \sin \theta}{\sqrt{1 - 2 \alpha \omega}}.$$

Let this value of y be substituted in the differential of the refraction; then

$$r^2 = (a + z)^2 = a^2 \left(1 + \frac{z}{a}\right)^2 = \frac{a^2}{\left(1 - \frac{\sigma}{a}\right)^2},$$

$$d . \delta \theta = \sin \theta \times \frac{\alpha}{1 - 2 \alpha \omega} \times \frac{d \omega}{\sqrt{\frac{1 - 2 \alpha \omega}{\left(1 - \frac{\sigma}{a}\right)^2} - \sin^2 \theta}}.$$

In further transforming this expression, it is to be observed that α is a very small fraction less than ·0003; and if the atmosphere extend fifty miles above the earth's surface, $\frac{z}{a}$ or $\frac{\sigma}{a}$ when greatest will not exceed ·012. If we now put

$$\frac{\sigma}{a} = \frac{s}{a} + \alpha \omega,$$

we shall have

$$\frac{1 - 2 \alpha \omega}{\left(1 - \frac{\sigma}{a}\right)^2} = \frac{(1 - \alpha \omega)^2 - \alpha^2 \omega^2}{\left(1 - \alpha \omega - \frac{s}{a}\right)^2} = 1 + 2 \frac{s}{a} + 3 \frac{s^2}{a^2},$$

the quantities rejected being plainly of no account relatively to those retained. Further, because ω is always less than 1, $\frac{\alpha}{1 - 2 \alpha \omega}$ is contained between α and $\alpha (1 + 2 \alpha)$; and

it may be taken equal to α, or to the mean value $\alpha(1+\alpha)$. Thus we have

$$d.\delta\theta = \sin\theta \times \frac{\alpha(1+\alpha)\,d\,u\,c^{-u}}{\sqrt{\cos^2\theta + 2\frac{s}{a} + 3\frac{s^2}{a^2}}}.$$

Again, the formula (9.) gives

$$\sigma = s + a.\alpha\omega = \frac{p'}{\rho'} \cdot \left\{u - f'.\frac{d.c^{-u}R_2}{c^{-u}du} - f'.\frac{dd.c^{-u}R_4}{c^{-u}.du^2} - \&c.\right\}.$$

Now,

$$\frac{p'}{\rho'} = \frac{p'}{\rho'(1+\beta\tau')} \cdot (1+\beta\tau') = L(1+\beta\tau'):$$

and if we make

$$s.\frac{\varrho'}{p'} = \frac{s}{L(1+\beta\tau')} = x,$$

$$\frac{p'}{\rho'}.\frac{1}{a} = \frac{L(1+\beta\tau')}{a} = i,$$

$$\frac{a.\rho'.\alpha}{p'} = \frac{\alpha}{i} = \lambda,$$

we shall have

$$\frac{s}{a} = i\,x$$

$$x = u - \lambda(1-c^{-u}) - f'.\frac{d.c^{-u}R_2}{c^{-u}du} - f'\frac{dd.c^{-u}R_4}{c^{-u}du^2} - \&c.$$

Let $\Psi(u)$ stand for all the terms in this value of x except the first, so that

$$x = u - \Psi(u):$$

from this we deduce by LAGRANGE's theorem,

$$c^{-u} = c^{-x} - c^{-x}\Psi(x) - \frac{1}{2}.\frac{d.c^{-x}\Psi^2(x)}{dx} - \&c.:$$

consequently,

$$du\,c^{-u} = dx\,c^{-x} + \frac{d.c^{-x}\Psi(x)}{dx}dx + \frac{1}{2}.\frac{dd.c^{-x}\Psi^2(x)}{dx^2}dx + \&c.$$

By means of the values that have been found, the differential of the refraction can be expressed in terms of one variable x. In making the substitutions, the smallest term of the radical quantity is to be neglected in all the terms of $d\,u\,c^{-u}$, except the first and greatest; and the denominator of that term is to be expanded. Thus we obtain

$$\begin{aligned} d.\delta\theta = \sin\theta.\alpha(1+\alpha).\Big\{ & \int\frac{dx}{\sqrt{\cos^2\theta + 2ix}}.\left(c^{-x} + \frac{d.c^{-x}\Psi(x)}{dx}\right) \\ & + \frac{1}{2}\int\frac{dx}{\sqrt{\cos^2\theta + 2ix}}.\frac{dd.c^{-x}\Psi^2(x)}{dx} \\ & - \frac{3}{2}\int\frac{dx.c^{-x}.i^2x^2}{(\cos^2\theta + 2ix)^{\frac{3}{2}}}. \end{aligned}$$

In order to estimate the relative magnitude of the several parts of this formula we

must find the numerical values of the quantities α and i. If η stands for the refraction at 45° of altitude, determined very exactly from many astronomical observations, we shall have

$$\alpha = \eta \, (1 - 2 i + 2 \eta),$$

as will readily appear from the formula according to CASSINI's method given in § 1. MM. BIOT and ARAGO have ascertained the value of α with great exactness in a different way, by means of experiments on the gases with the prism. In some of the best attempts to determine α, the refractions at 45° of altitude, being reduced to the barometer 29·6 and to the temperature 50° FAHR., are as follows:

Dr. BRINKLEY	57″·42
DE LAMBRE	57·58
BESSEL, Tab. Reg	57·55
Experiments of MM. BIOT and ARAGO	57·65
Mean	57·55

It appears that BESSEL's determination has the best claim to be preferred: but as it differs very little from DE LAMBRE's result, which is adopted in the paper of 1823, the same value will be retained in the calculations which follow. According to DE LAMBRE, the value of α is 60″·616* at the temperature 0° centigrade, and the barometric pressure $0^{m}\cdot76$: wherefore, when the temperature is 50° FAHR. and the pressure 30 inches ($= 0^{m}\cdot762$), we shall have

$$\alpha = 60{\cdot}616 \times \frac{762}{760 \times 1{\cdot}0018} \times \frac{1}{1 + \frac{18}{480}} = 58''{\cdot}47:$$

and in parts of the radius,

$$\alpha = {\cdot}0002835.$$

It has been found that L = 4347·8 fathoms at 0° centigrade or 32° FAHR.: wherefore, if we make a = mean radius of the earth = 3481280 fathoms, we shall have at the temperature of our climate, or 50° FAHR.,

$$i = \frac{L\,(1 + \beta\,\tau')}{a} = \frac{4347{\cdot}8 \left(1 + \frac{18}{483}\right)}{3481280} = {\cdot}0012958;$$

and hence

$$\lambda = \frac{\alpha}{i} = {\cdot}21878.$$

We can now inquire into the values of the last two terms of the foregoing formula for the refraction, both of which are very small. With respect to the first of them, we have,

$$\Psi(x) = \lambda\,(1 - c^{-x}) + f \cdot \frac{d\,.\,c^{-x}\,R_2}{c^{-x}\,d\,x} + f' \cdot \frac{d\,d\,.\,c^{-x}\,R_4}{c^{-x}\,d\,x^2} - \text{\&c.}:$$

* Tableaux Chronomiques, publiées par le Bureau des Longitudes de France.

and, by performing the differential operations,

$$\Psi(x) = \lambda(1 - c^{-x}) + f(R_1 + R_2) + f'(R_2 + 3R_3 + R_4);$$

and, by substituting the values of the functions,

$$h = 2f - \lambda = \cdot 22566$$

$$\Psi(x) = -h(1 - c^{-x}) + fx + 4f'\left(1 - x + \frac{3x^2}{8} - \frac{x^3}{24} - c^{-x}\right).$$

It might not be very objectionable to neglect the term multiplied by f', for the same reasons that the terms which follow it are neglected, that is, both on account of the nature of the functions and because the coefficients are small: but, in order to leave no room for scruples respecting accuracy, the square of the entire expression set down, may be thus represented:

$$\Psi^2(x) = G - 8hf' \cdot G' + 8ff' \cdot G'' + 16f'^2 \cdot G'''.$$

The integral in the term under consideration is greatest when the radical quantity in the denominator is least, that is, when $\cos\theta = 0$: and if the integration be performed betwen the limits $x = 0$, $x = \infty$, we shall obtain a result greater than if the integral were extended only to the top of the atmosphere. Now we have,

$$G = h^2(1 - 2c^{-x} + c^{-2x}) + 2hf \cdot xc^{-x} - 2hf \cdot x + f^2 \cdot x^2:$$

and, by operating on the terms separately, the part of the integral depending on G, will be as follows:

$$\int_0^\infty \frac{dx}{\sqrt{2ix}} \cdot \frac{dd \cdot c^{-x}G}{dx^2} =$$

$$\frac{\sqrt{\pi}}{\sqrt{2i}} \times \left(h^2(1 - 4\sqrt{2} + 3\sqrt{3}) - 3hf(\sqrt{2} - 1) + \frac{3}{4}f^2\right) = \frac{\sqrt{\pi}}{\sqrt{2i}} \times \cdot 00216.$$

The other parts depending on G′, G″, G‴ are complicated; but they are troublesome more on account of the number of terms they contain than from any difficulty in the integrations. The following results have been obtained:

$$8hf' \times \int_0^\infty \frac{dx}{\sqrt{2ix}} \cdot \frac{dd \cdot c^{-x}G'}{dx^2} = -f' \times \frac{\sqrt{\pi}}{\sqrt{2i}} \times \cdot 01759,$$

$$8ff' \times \int_0^\infty \frac{dx}{\sqrt{2ix}} \cdot \frac{dd \cdot c^{-x}G''}{dx^2} = -f' \times \frac{\sqrt{\pi}}{\sqrt{2i}} \times \cdot 02043,$$

$$16f'^2 \int_0^\infty \frac{dx}{\sqrt{2ix}} \cdot \frac{dd \cdot c^{-x}G'''}{dx^2} = +f'^2 \times \frac{\sqrt{\pi}}{\sqrt{2i}} \times \cdot 00855.$$

Collecting all the parts, the term sought is found, viz.

$$\frac{\alpha(+\alpha)}{2} \cdot \int_0^\infty \frac{dx}{\sqrt{2ix}} \cdot \frac{dd \cdot c^{-x}\Psi^2(x)}{dx^2} =$$

$$\frac{\alpha(1+\alpha)\sqrt{\pi}}{\sqrt{2i}} \times \left(\cdot 00108 - f' \times \cdot 00142 + f'^2 \times \cdot 00427\right).$$

To this must be added the other term, which, being integrated in the same circumstances, gives,

$$-\frac{3}{2}\cdot\int_0^\infty \frac{d\,x\,c^{-x}}{\sqrt{2\,i\,x}}\times\frac{i\,x}{2}=-\frac{3}{8}\cdot\frac{\sqrt{\pi}}{\sqrt{2\,i}}=-\frac{\sqrt{\pi}}{\sqrt{2\,i}}\times \cdot 00049.$$

It thus appears that the two small terms of the expression of the refraction are, together, equal to

$$\frac{\alpha\,(1+\alpha)\,\sqrt{\pi}}{\sqrt{2\,i}}\cdot\left(\cdot 00059 - f'\times\cdot 00142 + f'^2\times\cdot 00427\right):$$

and as

$$\frac{\alpha\,(1+\alpha)\,\sqrt{\pi}}{\sqrt{2\,i}}=2036''\cdot 5,$$

the greatest amount of both is about 1″.

The whole refraction will therefore be thus expressed:

$$d\,.\,\delta\,\theta=\sin\theta\times\alpha\,(1+\alpha)\int\frac{d\,x}{\sqrt{\cos^2\theta+2\,i\,x}}\cdot\left(c^{-x}+\frac{d\,.\,c^{-x}\,\Psi\,(x)}{d\,x}\right),$$

with the assurance that the error cannot exceed 1″. If we substitute what $\Psi\,(x)$ stands for, we shall have

$$d\,.\,\delta\,\theta=\sin\theta\times\alpha\,(1+\alpha)\times\int\frac{d\,x}{\sqrt{\cos^2\theta+2\,i\,x}}\times$$

$$\left(c^{-x}+\lambda\cdot\frac{d\,.\,(c^{-x}-c^{-2x})}{d\,x}+f\cdot\frac{d\,d\,.\,c^{-x}\,R_2}{d\,x^2}+f'\cdot\frac{d^3\,.\,c^{-x}\,R_4}{d\,x^3}+\&c.\right).$$

This expression being regular, it may be continued to any number of terms, and it has the advantage of being linear with respect to the coefficients. Adverting to what x stands for, it will appear that $L\times x$ is nearly equal to s, or to z, that is, to the elevation in the atmosphere; so that, if we suppose the greatest height of the atmosphere is $10\times L$, or about fifty miles, the greatest value of x will be 10; and all the integrals in the foregoing expression must be taken between the limits zero and 10. But the quantity c^{-x} is so small when x has increased to 8 or 10, that the results are not sensibly different whether the integrals be extended to those limits or be continued to infinity. By substituting the values of the functions, the expression of $\delta\,\theta$ will take this form:

$$\delta\,\theta=\sin\theta\times\alpha\,(1+\alpha)\times\left\{\int\frac{d\,x\,c^{-x}}{\sqrt{\cos^2\theta+2\,i\,x}}+\lambda\int\frac{d\,x\,(2\,c^{-2x}-c^{-x})}{\sqrt{\cos^2\theta+2\,i\,x}}\right.$$

$$-f\int\frac{d\,x}{\sqrt{\cos^2\theta+2\,i\,x}}\cdot\left(4\,c^{-2x}-3\,c^{-x}+x\,c^{-x}\right)$$

$$+f'\int\frac{d\,x}{\sqrt{\cos^2\theta+2\,i\,x}}\cdot\left(8\,c^{-2x}-8\,c^{-x}+7\,x\,c^{-x}-2\,x^2\,c^{-x}+\frac{x^3\,c^{-x}}{6}\right)$$

$$-f''\int\frac{d\,x}{\sqrt{\cos^2\theta+2\,i\,x}}\cdot\left(16\,c^{-2x}-16\,c^{-x}+16\,x\,c^{-x}-\frac{15}{2}\,x^2\,c^{-x}\right.$$

$$\left.+\frac{11}{6}\,x\,c^{-x}-\frac{5}{24}\,x^4\,c^{-x}+\frac{x^5\,c^{-x}}{120}\right).$$

In order to illustrate the rapidity with which the terms decrease, it may be proper to find the limit of $\delta\theta$, by making $\cos^2\theta = 0$, and integrating between the limits $x = 0$, $x = \infty$; which limit is not sensibly different from the refraction at the horizon. Now it will be found that, in the circumstances mentioned,

$$\delta\theta = \frac{\alpha(1+\alpha)\sqrt{\pi}}{\sqrt{2i}} \times \Big\{1 + \lambda\left(\sqrt{2} - 1\right) - f\left(2\sqrt{2} - \tfrac{5}{2}\right) + f'\left(4\sqrt{2} - \tfrac{91}{16}\right) - f''\left(8\sqrt{2} - \tfrac{2895}{256}\right) - \&c.:$$

or, in seconds,

$$\delta\theta = 2072''{\cdot}46 - f' \times 62{\cdot}4 - f'' \times 10''{\cdot}2 - \&c.$$

From this calculation it appears that the term multiplied by f'' and all the subsequent terms are too small to be sensible; and as f' is much less than f, even the term multiplied by f' can hardly exceed a few seconds at low altitudes. There is great probability that the horizontal refraction is very near 34′ 30″, and does not exceed this quantity.

To prepare the foregoing expression of $\delta\theta$ for integration, put

$$m = 10, \quad \frac{\sqrt{2im}}{\cos\theta} = \tan\varphi, \quad e = \tan\frac{\varphi}{2};$$

then

$$\cos^2\theta = \frac{(1-e^2)^2}{4e^2} \times 2im,$$

$$\sqrt{\cos^2\theta + 2im} = \frac{\sqrt{5i}}{e}\cdot\sqrt{(1-e^2)^2 + 4e^2\cdot\frac{x}{m}} = \frac{\sqrt{5i}}{e}\cdot\Delta:$$

and we shall have

$$\left.\begin{aligned}\delta\theta = \sin\theta \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \times \Big\{&\int_0^m \frac{e\,dx}{\Delta}\cdot c^{-x}\\ &+\lambda\int_0^m \frac{e\,dx}{\Delta}\cdot\left(2c^{-2x} - c^{-x}\right)\\ &-f\int_0^m \frac{e\,dx}{\Delta}\cdot\left(4c^{-2x} - 3c^{-x} + xc^{-x}\right)\\ &+f'\int_0^m \frac{e\,dx}{\Delta}\cdot\left(8c^{-2x} - 8c^{-x} + 7xc^{-x} - 2x^2c^{-x} + \frac{x^3}{6}c^{-x}\right).\end{aligned}\right\} \quad (C.)$$

For the sake of abridging, the several integrals in succession may be represented by Q_0, Q_1, Q_2, Q_3; so that the value of $\delta\theta$ will be thus written:

$$\delta\theta \sin\theta \times \frac{\alpha(1+\alpha)}{\sqrt{5i}}\cdot(Q_0 + \lambda Q_1 - fQ_2 + f'Q_3).$$

10. The equation (C.) supposes that the atmosphere consists entirely of dry air: we have next to consider what modification must be made when it contains a portion of aqueous vapour.

In the first place, when p' and τ', the pressure and temperature at the surface of the earth, are given, as they are in the mean atmosphere which produces the refractions, the quantity α, or the refractive power of the air, is not liable to be altered by any possible mixture of aqueous vapour. For if an addition of vapour to dry air diminish the refractive power by making the density less, the greater action of the vapour upon light is found almost exactly to compensate the defect. LAPLACE first made this observation; which has been confirmed by MM. BIOT and ARAGO, who have established by experiments, that the refractive power of air, whether dry or mixed with vapour, is the same, when the pressure and temperature are the same. It thus appears that, as far as the quantity α, or the refractive power of the air at the earth's surface, is concerned, the astronomical refractions are independent on the hygrometric condition of the atmosphere.

But a mixture of vapour may produce changes in the expression of the refraction, by altering the coefficients or the integrals. Now, if we attend to the formulas that have been found for an atmosphere of moist air, and in the equation (10.) make the same substitution as in the case of dry air, viz.

$$\frac{\sigma}{a} = \frac{s}{a} + \alpha\,\omega,$$

we shall obtain

$$s \cdot \frac{(\rho')}{p'} = \frac{s\left(1 - \frac{3}{8}\frac{\phi'}{p'}\right)}{\mathrm{L}(1 + \beta\,\tau')} = x,$$

$$\frac{p'}{(\rho')} \cdot \frac{1}{a} = \frac{\mathrm{L}(1 + \beta\,\tau')}{a\left(1 - \frac{3}{8} \cdot \frac{\phi'}{p'}\right)} = i,$$

$$\alpha \cdot \frac{a\,(\rho')}{p'} = \frac{\alpha}{i} = \frac{a\left(1 - \frac{3}{8} \cdot \frac{\phi'}{p'}\right)}{\mathrm{L}(1 + \beta\,\tau')} = \lambda:$$

and further, it will appear that the same relation subsists between x and u in the atmosphere of moist air as between the quantities represented by the same letters in the atmosphere of dry air. The same procedure will therefore lead, in both cases, to the same integrals extending between the same limits. The only difference lies in the values of λ and i, which in the case of moist air acquire, as a multiplier or divisor, the small factor $\left(1 - \frac{3}{8} \cdot \frac{\phi'}{p'}\right)$ depending on the tension of the vapour at the earth's surface. If the hygrometer afforded an easy practical method of ascertaining the tension of the vapour, the minute variations of the refractions, arising from moisture in the atmosphere, might be corrected by the method usually employed for compen-

sating the small changes which a difference of temperature causes in the mean constants.

Experience confirms what has been said; for all the astronomers who have attended to aqueous vapour in the atmosphere, agree in admitting that it either has no influence, or but a very small and imperceptible effect, to alter the refractions. On this head it will be sufficient to cite the authority of M. Biot*, who seems carefully to have studied this point, on which he expresses himself very strongly. The very exact coincidence of the theoretical with the observed refractions as far as 88° or $88\frac{1}{2}°$ from the zenith, concurs to prove that the variable quantity of vapour in the air has little influence so long as it retains the gaseous form; but at lower altitudes, when the rays of light become almost parallel to the horizon, it is very probable that particular and local causes may come into play.

11. Nothing is now wanted for completing the solution of the problem, except the reducing of the expression (C.) to a form fit for numerical calculation.

Investigation of the integral Q_j.

We have

$$Q_0 = \int_0^m \frac{e\,d\,x\,c^{-x}}{\Delta} = \int_0^m \frac{e\,d\,x\,c^{-x}}{\sqrt{(1-e^2)^2 + 4\,e^2 \cdot \frac{x}{m}}}:$$

assume,

$$\Delta = \sqrt{(1-e^2)^2 + 4\,e^2 \cdot \frac{x}{m}} = 1 - e^2 + 2\,e^2\,z;$$

then,

$$\frac{e\,d\,x}{\Delta} = e \cdot m\,d\,z,$$

$$\frac{x}{m} = t = 1 - e^2\,(z - z^2).$$

By Lagrange's theorem,

$$\Psi = t - t^2,$$

$$z = t + e^2 \cdot \Psi + \frac{e^4}{1 \cdot 2} \cdot \frac{d \cdot \Psi^2}{d\,t} + \frac{e^6}{1 \cdot 2 \cdot 3} \cdot \frac{d\,d \cdot \Psi^3}{d\,t^2} + \&c.;$$

$$m\,d\,z = m\,d\,t \cdot \left\{1 + e^2 \cdot \frac{d \cdot \Psi}{d\,t} + \frac{e^4}{1 \cdot 2} \cdot \frac{d^2\,\Psi^2}{d\,t^2} + \frac{e^6}{1 \cdot 2 \cdot 3} \cdot \frac{d^3\,\Psi^3}{d\,t^3} + \&c.\right\};$$

consequently,

$$\int_0^m \frac{e\,d\,x\,c^{-x}}{\Delta} = \int_0^1 m\,d\,t\,c^{-mt}\left\{e + e^3\frac{d\Psi}{d\,t} + \frac{e^5}{1 \cdot 2} \cdot \frac{d^2\,\Psi^2}{d\,t^2} + \&c.\right\}.$$

Wherefore, if we assume

$$Q_0 = A_1\,e + A_3\,e^3 + A_5\,e^5 + \&c.,$$

* Précis Elem. de Physique, p. 229. tom. ii. edit. 2nd. Addit. à la Conn. des Temps, 1839, p. 36.

we shall have

$$A_{2n+1} = \frac{1}{1.2.3\ldots n}\int_0^1 m\,d\,t\,c^{-mt} \cdot \frac{d^n . \Psi^n}{d\,t^n}.$$

In the first place, it may be proper to show that all the coefficients in the series for Q^0 are positive. For this purpose integrate by parts, and the results will be

$$A_{2n+1} = \frac{m}{1.2.3\ldots n} \times \left\{ c^{-mt} \cdot \frac{d^{n-1}\Psi^n}{d\,t^{n-1}} + \int m\,d\,t\,c^{-mt} \cdot \frac{d^{n-1}\Psi^n}{d\,t^{n-1}} \right\}.$$

Now it is evident that

$$\frac{d^{n-1}\Psi^n}{d\,t^{n-1}}$$

is divisible both by t and $1 - t$: it is therefore zero at both the limits of the integral; so that we have simply

$$A_{2n+1} = \frac{m}{1.2.3\ldots n} \cdot \int_0^1 m\,d\,t\,c^{-mt} \cdot \frac{d^{n-1}\Psi^n}{d\,t^{n-1}}.$$

Continuing to integrate in like manner, we shall find after n successive operations,

$$A_{2n+1} = \frac{m^n}{1.2.3\ldots n}\int_0^1 m\,d\,t\,c^{-mt}\,\Psi^n,$$

which is obviously a positive quantity.

By expanding, we get

$$\Psi^n = t^n (1-t)^n = t^n - n . t^{n+1} + n \cdot \frac{n-}{2} \cdot t^{n+2} - \&c.:$$

and, by performing the differential operations,

$$\frac{1}{1.2.3\ldots n} \cdot \frac{d^n\Psi^n}{d\,t^n} = 1 - n . n+1 \cdot \frac{t}{1} + \left(n \cdot \frac{n-1}{2}\right) \cdot n+1 . n+2 \cdot \frac{t^2}{1.2} - \&c.$$

Now, because $t = \frac{x}{m}$, if we put,

$$\Psi'(x) = \frac{1}{1.2.3\ldots n} \cdot \frac{d^n\Psi^n}{d\,t^n},$$

we shall have

$$\Psi'(x) = 1 - n \cdot \frac{n+1}{m} \cdot \frac{x}{1} + n \cdot \frac{n-1}{2} \cdot \frac{n+1 . n+2}{m^2} \cdot \frac{x^2}{1.2} - \&c.$$

Another form may be given to this function; for, without any variation in quantity, t and $1 - t$ may be interchanged, not only in

$$\Psi^n = t^n (1-t)^n,$$

but in all its differentials, observing that the results equal in quantity will have opposite signs when the number of differentiations is odd, and the same sign when the number is even. Now if, instead of $t = \frac{x}{m}$, we substitute $1 - t = \frac{m-x}{m}$, we shall have

$$\Psi'(x) = \pm \left\{ 1 - n \cdot \frac{n+1}{m} \cdot \frac{m-x}{1} + n \cdot \frac{n-1}{2} \cdot \frac{n+1 . n+2}{m^2} \cdot \frac{(m-x)^2}{1.2} - \&c. \right.$$

The coefficient A_{2n+1} is thus expressed in terms of x:

$$A_{2n+1} = \int_0^m dx\, c^{-x}\, \Psi'(x):$$

the indefinite integral is

$$- c^{-x} . \left\{ \Psi'(x) + \frac{d . \Psi'(x)}{dx} + \frac{dd . \Psi'(x)}{dx^2} + \&c. \right\}.$$

This integral, taken between the limits $x = 0$ and $x = m$, is equal to A_{2n+1}: the first form of $\Psi'(x)$ will give the values of all the differentials at the limit $x = 0$; and the second form of the same function will give the like values at the other limit $x = m$: Thus we obtain,

$$A_{2n+1} \left\{ 1 - n \cdot \frac{n+1}{m} + n \cdot \frac{n-1}{2} \cdot \frac{n+1 . n+2}{m^2} - \&c. \right\}$$

$$\mp c^{-m} . \left\{ 1 + n \cdot \frac{n+1}{m} + n \cdot \frac{n-1}{2} \cdot \frac{n+1 . n+2}{m^2} + \&c. \right\},$$

the upper or lower sign taking place according as n is even or odd.

The numerical coefficients, computed by the formula, are as follows:

$$c^{-m} = c^{-10} = \cdot 0000454$$

$$A_1 = 1 - c^{-m} = 0\cdot 9999546$$

$$A_3 = \frac{4}{5} + \frac{6}{5} c^{-m} = 0\cdot 8000545$$

$$A_5 = \frac{13}{25} - \frac{43}{25} c^{-m} = 0\cdot 5199219$$

$$A_7 = \frac{7}{25} + \frac{73}{25} c^{-m} = 0\cdot 2801326$$

$$A_9 = \frac{16}{125} - \frac{726}{125} c^{-m} = 0\cdot 1277363$$

$$A_{11} = \frac{31}{625} + \frac{8359}{625} c^{-m} = 0\cdot 0502072$$

$$A_{13} = 0\cdot 0172805$$

$$A_{15} = 0\cdot 0052779$$

$$A_{17} = 0\cdot 0014467$$

$$A_{19} = 0\cdot 0003593$$

$$A_{21} = 0\cdot 0000815$$

$$A_{23} = 0\cdot 0000170$$

$$A_{25} = 0\cdot 0000036.$$

The horizontal refraction answers to $\cos \theta = 0$, $e = 1$; and the part of it depending on Q_0 is found by adding all the coefficients, viz.

$$\frac{\alpha (1 + \alpha)}{\sqrt{5i}} \times 2\cdot 8024736 = 2036''\cdot 52.$$

If we take the integral between the limits $x = 0$, $x = \infty$, the result is not sensibly different, viz.

$$\alpha(1+\alpha)\int_0^{\infty} \frac{d x\, c^{-x}}{\sqrt{2 i x}} = \frac{\alpha(1+\alpha)\sqrt{\pi}}{\sqrt{2 i}} = 2036''\cdot 52.$$

Investigation of $\lambda \times Q_1$.

For this purpose we must find the value of

$$\int_0^m \frac{2 d x\, c^{-2x}}{\Delta} = \int_0^m \frac{2 d x\, c^{-2x}}{\sqrt{(1-e^2)^2 + 4 e^2 \cdot \frac{x}{m}}} = \int_0^m \frac{2 d x\, c^{-2x}}{\sqrt{(1-e^2)^2 + 4 e^2 \cdot \frac{2x}{2m}}}:$$

this integral has therefore the same form as Q_0, the quantities $2x$ and $2m$ taking the place of x and m. Wherefore, if we assume

$$e \times \int \frac{2 d x\, c^{-2x}}{\Delta} = a_1 e + a_3 e^3 + a_5 e^5 + \&c.,$$

the value of a_{2n+1} will be found merely by writing $2m$ for m in the expression of A_{2n+1}; but as $c^{-2m} = c^{-20}$ is extremely minute, the part multiplied by it may be neglected. Thus,

$$a_{2n+1} = 1 - n \cdot \frac{n+1}{2m} + n \cdot \frac{n-1}{2} \cdot \frac{n+1 \,.\, n+2}{(2m)^2} - \&c.$$

The numerical coefficients are as follows:

$$\begin{aligned}
a_1 &= 1\\
a_3 &= 0\cdot 9\\
a_5 &= 0\cdot 73\\
a_7 &= 0\cdot 535\\
a_9 &= 0\cdot 3555\\
a_{11} &= 0\cdot 21505\\
a_{13} &= 0\cdot 118945\\
a_{15} &= 0\cdot 0604215\\
a_{17} &= 0\cdot 0283127\\
a_{19} &= 0\cdot 0122898\\
a_{21} &= 0\cdot 0049621\\
a_{23} &= 0\cdot 0018695\\
a_{25} &= 0\cdot 0006623.
\end{aligned}$$

These values being found, if we assume

$$\lambda \times Q_1 = B_3 e^3 + B_5 e^5 + B_7 e^7 + \&c.,$$

the term $\lambda \times c^{-m} \times e$, which is insensible, being omitted, we shall have

$$B_3 = \lambda(a_3 - A_3) = 0\cdot 021866$$

$$B_5 = \lambda\ (a_5 - A_5) = 0{\cdot}045961$$
$$B_7 = \lambda\ (a_7 - A_7) = 0{\cdot}055760$$
$$B_9 = \lambda\ (a_9 - A_9) = 0{\cdot}049829$$
$$B_{11} = \lambda\ (a_{11} - A_{11}) = 0{\cdot}036064$$
$$B_{13} = \lambda\ (a_{13} - A_{13}) = 0{\cdot}022242$$
$$B_{15} = \lambda\ (a_{15} - A_{15}) = 0{\cdot}012064$$
$$B_{17} = \lambda\ (a_{17} - A_{17}) = 0{\cdot}005878$$
$$B_{19} = \lambda\ (a_{19} - A_{19}) = 0{\cdot}002610$$
$$B_{21} = \lambda\ (a_{21} - A_{21}) = 0{\cdot}001067$$
$$B_{23} = \lambda\ (a_{23} - A_{23}) = 0{\cdot}000405$$
$$B_{25} = \lambda\ (a_{25} - A_{25}) = 0{\cdot}000144.$$

By making $\cos\theta = 0$, $e = 1$, we shall have, for the approximate value of the part of the horizontal refraction depending on $\lambda\, Q_1$,

$$\frac{\alpha(1+\alpha)}{\sqrt{5i}} \times 0{\cdot}253891 = 184''{\cdot}50.$$

If the integrals be taken from $x = 0$ to $x = \infty$, the same quantity will be

$$\lambda \times \alpha(1+\alpha) \int \frac{dx\,(2c^{-2x} - c^{-x})}{\sqrt{2ix}} = \frac{\alpha(1+\alpha)\sqrt{\pi}}{\sqrt{2i}} \cdot \lambda\,(\sqrt{2} - 1) = 184''{\cdot}56.$$

Between the two limits, the exact quantity obtained by integrating from $x = 0$ to $x = m = 10$, must lie; so that the error of the series is of no account.

It may be proper to make an observation here, which applies generally to the kind of integrals peculiar to this investigation. The first term of $\lambda\, Q_1$, viz. $B_1 e = \lambda\, c^{-m} . e$, which is rejected, varies with the height of the atmosphere. If a small number be taken for m, that is, in low atmospheres, the refractions will vary with the height, and will not agree with the observed quantities; if a considerable number be taken, as eight or ten, or any greater number, that is, if the atmosphere extend forty or fifty miles or more above the earth's surface, the refractions will not be sensibly different from what they would be in an atmosphere of unlimited height. The invariability of the refractions concurs with other phenomena to prove that the air reaches an elevation of fifty miles, more or less.

Investigation of $f \times Q_2$.

We have

$$\frac{Q_2}{e} = \int_0^m \frac{dx}{\Delta}\left(4c^{-2x} - 3c^{-x} + xc^{-x}\right).$$

Now the following formula is easily proved by differentiating,

$$\int \frac{dx}{\Delta} x c^{-x} = \frac{1}{2}\int \frac{dx\, c^{-x}}{\Delta} - \frac{m}{4} \cdot \frac{(1-e^2)^2}{e^2} \cdot \int \frac{dx\, c^{-x}}{\Delta} + \frac{m}{4} \cdot \frac{1 - e^2 - c^{-x}\Delta}{e^2},$$

all the integrals vanishing when $x = 0$. By extending the integrals to $x = m = 10$, in which case $\Delta = 1 + e^2$, the result will be

$$\int_0^m \frac{dx}{\Delta} x c^{-x} = \frac{1}{2}\int_0^m \frac{dx\, c^{-x}}{\Delta} - \frac{5}{2} \cdot \frac{(1-e^2)^2}{e^2} \cdot \int_0^m \frac{dx\, c^{-x}}{\Delta} + \frac{5}{2} \cdot \frac{1 - e^2 + c^{-m}(1+e^2)}{e^2}:$$

and, by substituting this value, we shall have

$$Q_2 = 2e\int_0^m \frac{2\,dx\,c^{-2x}}{\Delta} - \frac{5}{2}\int_0^m \frac{e\,dx\,c^{-x}}{\Delta} - \frac{5}{2} \cdot \frac{(1-e^2)^2}{e^2} \cdot \int_0^m \frac{e\,dx\,c^{-x}}{\Delta}$$
$$+ \frac{5}{2} \cdot \frac{1 - c^{-m}}{e} - \frac{5}{2}(1 + c^{-m})\, e.$$

The value of Q_2 will now be obtained in a series of the powers of e by putting for the integrals the equivalent series that have already been investigated. When this is done, the three first terms will be as follows:

$$\frac{5}{2}\left(1 - c^{-m} - A_1\right) \cdot \frac{1}{e}$$
$$+ \left(2\, a_1 - \frac{5}{2} A_1 + 5\, A_1 - \frac{5}{2} A_3 - \frac{5}{2}(1 + c^{-m})\right) \cdot e$$
$$+ \left(2\, a_3 - \frac{5}{2} A_3 - \frac{5}{2} A_1 + 5\, A_3 - \frac{5}{2} A_5\right) \cdot e^3.$$

Upon substituting the exact values of A_1, A_3, &c., the first of these terms is zero: the other two are as follows:

$$- 8\, c^{-m} \times e$$
$$+ \frac{49}{5}\, c^{-m} \times e^3;$$

the amount of which is very small even at the horizon; and, when multiplied by $f = \frac{2}{9}$, it becomes insensible. These terms being neglected, we may assume

$$Q_2 = C_5\, e^5 + C_7\, e^7 + C_9\, e^9 + \&c.;$$

and we shall find

$$C_5 = 2\, a_5 - \frac{5}{2} A_5 - \frac{5}{2} \Delta^2 A_3$$

$$C_7 = 2\, a_7 - \frac{5}{2} A_7 - \frac{5}{2} \Delta^2 A_5$$

.

.

.

$$C_{2n+1} = 2\, a_{2n+1} - \frac{5}{2} A_{2n+1} - \frac{5}{2} \Delta^2 A_{2n-1}.$$

The numerical coefficients will now be obtained:

$$\Delta^2 A_1 = - \cdot 0802325$$
$$\Delta^2 A_3 = + \cdot 0403433 \;\ldots\ldots\; C_5 = \cdot 059337$$

$\Delta^2 A_5 =$ ·0873930 . . . . . $C_7 =$ ·151186

$\Delta^2 A_7 =$ ·0748672 . . . . . $C_9 =$ ·204491

$\Delta^2 A_9 =$ ·0446024 . . . . . $C_{11} =$ ·193076

$\Delta^2 A_{11} =$ ·0209241 . . . . . $C_{13} =$ ·142381

$\Delta^2 A_{13} =$ ·0081714 . . . . . $C_{15} =$ ·087220

$\Delta^2 A_{15} =$ ·0027438 . . . . . $C_{17} =$ ·046149

$\Delta^2 A_{17} =$ ·0008096 . . . . . $C_{19} =$ ·021658

$\Delta^2 A_{19} =$ ·0002133 . . . . . $C_{21} =$ ·009187

$\Delta^2 A_{21} =$ ·0000511 . . . . . $C_{23} =$ ·003569

$\Delta_2 A_{23} =$ ·0000105 . . . . . $C_{25} =$ ·001290.

As the value of f is not fixed with the same certainty as that of λ, the coefficients of Q_2 have not been multiplied by f: the intention of which is to make it more easy to determine a variation of the refraction, viz $\delta f \times Q_2$, answering to δf any variation of f that good observations may require.

The part of the horizontal refraction depending on Q_2 is

$$\frac{2}{9} \times \frac{\alpha (1 + \alpha)}{\sqrt{5i}} \times 0{\cdot}919534 = 148''{\cdot}51.$$

If we integrate the original expression of Q_2 from $x = 0$ to $x = \infty$, e being 1, we shall have

$$\frac{2}{9} \times \alpha (1 + \alpha) \times \int \frac{dx (4c^{-2x} - 3c^{-x} + xc^{-x})}{\sqrt{2ix}} = \frac{2}{9} \cdot \frac{\alpha (1 + \alpha) \sqrt{\pi}}{\sqrt{2i}} \cdot \left(2\sqrt{2} - \frac{5}{2}\right) = 148''{\cdot}63.$$

It thus appears that the error is less than $0''{\cdot}12$; for the exact integral from $x = 0$ to $x = m = 10$, is less than the second number, and greater than the first on account of the terms of the series left out.

The next point that should engage attention is to find the value of $f' \times Q_3$. In the present state of our knowledge of the phenomena of the atmosphere, it seems impossible to determine f' by experiments. The probability is, that it is much less than f or $\frac{2}{9}$; and as the integral Q_3 is inconsiderable except within a degree or two above the horizon, and even at such low altitudes is not great; it follows that the part of the refraction depending on $f' Q_3$ will only be sensible, if at all, when a star is distant 88° or more from the zenith. At present the probability is, that there is no other way of ascertaining the value of f' but by good observed refractions at great distances from the zenith; which observations are neither numerous nor easily collected. From the uncertainty of the term $f' \times Q_3$, it cannot be estimated in constructing a table of mean refractions, which must therefore be deduced entirely from the other three terms, as in the paper of 1823. In this manner has the table in this paper been computed, by means of the formulas now to be explained. But the term $f' Q_3$ will after-

wards be discussed, and its value investigated, in order that it may be taken into account, if this should be found necessary, in the progressive improvement of the theory.

When the term $f' Q_3$ is left out, the expression of the refraction will be

$$\delta \theta = \sin \theta \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \cdot (Q_0 + \lambda Q_1 - f Q_2):$$

and if the equivalent series be substituted for the first two terms, and the series for Q_2 be multiplied by $f = \frac{2}{9}$, the result will be

$$\delta \theta = \sin \theta \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \times \left\{ \begin{array}{l} e \\ + 0{\cdot}821921 \,.\, e^3 \\ + 0{\cdot}552697 \,.\, e^5 \\ + 0{\cdot}302296 \,.\, e^7 \\ + 0{\cdot}132123 \,.\, e^9 \\ + 0{\cdot}043365 \,.\, e^{11} \\ + 0{\cdot}007883 \,.\, e^{13} \\ - 0{\cdot}002040 \,.\, e^{15} \\ - 0{\cdot}002930 \,.\, e^{17} \\ - 0{\cdot}001842 \,.\, e^{19} \\ - 0{\cdot}000893 \,.\, e^{21} \\ - 0{\cdot}000371 \,.\, e^{23} \\ - 0{\cdot}000139 \,.\, e^{25} \end{array} \right.$$

To bring this formula to a form more convenient for calculation, all the coefficients must be reduced to seconds. The negative terms are all very small, never amounting to so much as $6''$, and of no account whatever, except the apparent altitude be equal to $2°$ or less; it will therefore be proper to separate these terms from the rest, representing their sum by the symbol $V(\theta)$. These things being attended to, we have, in the first place, this formula for computing e, viz.

$$\log . \tan \varphi = \log . \sec . \theta + 19{\cdot}2067840 - 20: \; e = \tan \frac{\varphi}{2}.$$

Next, reducing the arcs to seconds,

$$\frac{\alpha(1+\alpha)}{\sqrt{5i}} = 726''{\cdot}687:$$

$$\delta \theta = \sin \theta \times \left\{ \begin{array}{ll} & \text{log.} \\ e \times 726''{\cdot}687, & 2{\cdot}8613472 \\ + e^3 \times 597{\cdot}280, & 2{\cdot}7761772 \\ + e^5 \times 401{\cdot}638, & 2{\cdot}6038343 \end{array} \right.$$

$$
\begin{aligned}
&+ e^7 \times 219{\cdot}674, \quad 2{\cdot}3417796\\
&+ e^9 \times 96{\cdot}012, \quad 1{\cdot}9823255\\
&+ e^{11} \times 31{\cdot}513, \quad 1{\cdot}4984866\\
&+ e^{13} \times 5{\cdot}728, \quad 0{\cdot}7580287\\
&- \mathrm{V}(\theta).
\end{aligned}
$$

$$
\begin{aligned}
\mathrm{V}(\theta) = \sin\theta \times \Big\{ & e^{15} \times \overset{''}{1}{\cdot}483, \quad \overset{\log.}{0{\cdot}1710}\\
&+ e^{17} \times 2{\cdot}129, \quad 0{\cdot}3282\\
&+ e^{19} \times 1{\cdot}337, \quad 0{\cdot}1266\\
&+ e^{21} \times 0{\cdot}649, \quad -1{\cdot}8122\\
&+ e^{23} \quad 0{\cdot}270, \quad -1{\cdot}4307\\
&+ e^{25} \quad 0{\cdot}102, \quad -1{\cdot}0072
\end{aligned}
$$

When $\theta = 87°$, $\mathrm{V}(\theta)$ is zero; and if this function be computed for every succeeding half-degree, the quantity answering to any intermediate value of θ will be found by an easy interpolation. Such is the intention of the following Table; by the help of which any refraction from the zenith to the horizon may be computed by a series of the simplest form, and consisting of no more than seven terms.

θ.	$\mathrm{V}(\delta\,\theta.)$
$87\frac{1}{2}$	$\overset{''}{0}{\cdot}06$
88	0·14
$88\frac{1}{2}$	0·38
89	0·86
$89\frac{1}{2}$	2·30
90	5·97

If $e = 1$, the result will be the horizontal refraction, viz.

$$2078''{\cdot}53 - 5''{\cdot}97 = 2072''{\cdot}56,$$

which is almost exactly the same with $2072''{\cdot}46$, the quantity before computed in § 10 by a very different method.

12. We next proceed to inquire into the influence which the term multiplied by f', before omitted, may have on the refractions.

Investigation of the integral Q_3.

The expression of this integral is,

$$\mathrm{Q}_3 = \int_0^m \frac{e\,dx}{\Delta}\left(8\,c^{-2x} - 8\,c^{-x} + 7\,x\,c^{-x} - 2\,x^2\,c^{-x} + \frac{x^3}{6}\,c^{-x}\right),$$

which is a negative quantity, as appears from the valuation of it in § 9: it will therefore contribute to distinctness if its sign be changed, in which case it will be thus

written,

$$Q_3 = \int_0^m \frac{e\,dx}{\Delta}\left(-8c^{-2x} + 8c^{-x} - 7xc^{-x} + 2x^2c^{-x} - \frac{x^3}{6}c^{-x}\right);$$

and the formula for the refractions will now be,

$$\delta\theta = \sin\theta \times \frac{\alpha(1+\alpha)}{\sqrt{5i}}\left(Q_0 + \lambda Q_1 - f Q_2 - f' Q_3\right).$$

Suppressing the tedious operations of reducing, we may put the integral Q_3, taken indefinitely, in the following form, which it is not difficult to verify by differentiating:

$$\varepsilon = \frac{1-e^2}{e},$$

$$\begin{aligned} Q_3 = & -4\int\frac{e.2dx\,c^{-2x}}{\Delta} + \frac{91}{16}\int\frac{e\,dx\,c^{-x}}{\Delta} \\ & + \left(\frac{215}{16}\varepsilon^2 + \frac{175}{16}\varepsilon^4 + \frac{125}{48}\varepsilon^6\right).\int\frac{e\,dx\,c^{-x}}{\Delta} \\ & + \frac{c^{-x}\Delta}{e}\left(\frac{185}{16} + \frac{125}{12}\varepsilon^2 + \frac{125}{48}\varepsilon^4\right) \\ & - \varepsilon\left(\frac{185}{16} + \frac{125}{12}\varepsilon^2 + \frac{125}{48}\varepsilon^4\right) \\ & - \frac{c^{-x}\Delta}{e}\left(\frac{95}{24}x - \frac{5}{12}x^2 + \frac{25}{24}x\,.\,\varepsilon^2\right). \end{aligned}$$

This being the indefinite integral, the value of Q_3 in the formula for the refractions will be obtained by putting $x = m = 10$; which gives

$$\frac{c^{-x}\Delta}{e} = \frac{1+e^2}{e}\cdot c^{-m}:$$

and this value, as well as that of ε, being substituted, the quantity sought will be expressed as follows:

$$\begin{aligned} Q_3 = & -4\int\frac{e.2dx\,c^{-2x}}{\Delta} + \frac{91}{16}\int\frac{e\,dx\,c^{-x}}{\Delta} \\ & + \left\{\frac{215}{16}\left(\frac{1-e^2}{e}\right)^2 + \frac{175}{16}\left(\frac{1-e^2}{e}\right)^4 + \frac{125}{48}\left(\frac{1-e^2}{e}\right)^6\right\}.\int\frac{e\,dx\,c^{-x}}{\Delta} \\ & + c^{-m}\left(\frac{125}{48}\frac{1}{e^5} - \frac{125}{16}\frac{1}{e^3} + \frac{905}{48}\cdot\frac{1}{e} + \frac{905}{48}e - \frac{125}{16}e^3 + \frac{127}{48}e^5.\right) \\ & - \frac{125}{48}\cdot\frac{1}{e^5} + \frac{125}{48}\frac{1}{e^3} - \frac{305}{48}\frac{1}{e} + \frac{305}{48}e - \frac{125}{48}e^3 + \frac{125}{48}e^5. \end{aligned}$$

The series equivalent to the integrals must now be substituted, in order to express the quantity sought in terms containing the powers of e.

In the first place we have these three terms, each of which is zero when the exact values of A_1, A_3, &c. are substituted, viz.

$$\frac{125}{48}(A_1 - 1 + c^{-m})\cdot\frac{1}{e}$$

$$+\left\{\frac{175}{16}A_1+\frac{125}{48}(A_3-6A_1)+\frac{125}{48}-\frac{125}{16}c^{-m}\right\}.\frac{1}{e^3}$$

$$+\left\{\frac{215}{16}A_1+\frac{175}{16}(A_3-4A_1)+\frac{125}{48}(A_5-6A_3+15A_1)-\frac{305}{48}+\frac{905}{48}c^{-m}\right\}\times\frac{1}{e^5}$$

The next three terms are as follows:

$$\left\{-4a_1+\frac{91}{16}A_1+\frac{215}{16}(A_3-2A_1)+\frac{175}{16}(A_5-4A_3+6A_1)\right.$$
$$\left.+\frac{125}{48}(A_7-6A_5+15A_3-20A_1)+\frac{305}{48}+\frac{905}{48}c^{-m}\right\}.e$$

$$+\left\{-4a_3+\frac{91}{16}A_3+\frac{215}{16}(A_5-2A_3+A_1)\right.$$
$$+\frac{175}{16}(A_7-4A_5+6A_3-4A_1)$$
$$+\frac{125}{48}(A_9-6A_7+15A_5-20A_3+15A_1)$$
$$\left.-\frac{125}{48}-\frac{125}{16}c^{-m}\right\}.e^3$$

$$+\left\{-4a_5+\frac{91}{16}A_5+\frac{215}{16}(A_7-2A_5+A_3)\right.$$
$$+\frac{175}{16}(A_9-4A_7+6A_5-4A_3+A_1)$$
$$+\frac{125}{48}(A_{11}-6A_9+15A_7-20A_5+15A_3-6A_1)$$
$$\left.+\frac{125}{48}+\frac{125}{48}c^{-m}\right\}.e^5.$$

On substituting the exact values of A_1, A_3, &c., these three terms will come out as follows:

$$+\frac{158}{3}c^{-m}.e,\qquad \text{or } +{\cdot}00239\,.\,e$$

$$-\frac{348}{5}c^{-m}.e^3,\qquad \text{or } -{\cdot}00316\,.\,e^3$$

$$+\frac{8891}{75}c^{-m}.e^5,\qquad \text{or } +{\cdot}00538\,.\,e^5.$$

These three terms are the part of the refraction that depends on the height of the atmosphere: at the horizon, or when $e=1$, their amount is greatest and equal to

$$f'\times\frac{\alpha(1+\alpha)}{\sqrt{5i}}\times{\cdot}00461=f'\times 726''{\cdot}7\times{\cdot}00461=f'\times 3''{\cdot}3,$$

which, on account of the smallness of f', will be a minute fraction of a second.

Rejecting the six foregoing terms, we may assume

$$Q_3=H_7e^7+H_9e^9+H_{11}e^{11}+\&c.:$$

and, having computed the differences in the following table,

	Δ^2	Δ^4	Δ^6
A_1	....	....	+·0278859
A_3	....	—·0595755	—·0175110
A_5	+·0873930	—·0177390	—·0199864
A_7	+·0748672	+·0065865	—·0079396
A_9	+·0446024	+·0109256	—·0002312
A_{11}	+·0209241	+·0073251	+·0016762
A_{13}	+·0081714	+·0034934	+·0012515
A_{15}	+·0027438	+·0013379	+·0005925
A_{17}	+·0008096	+·0004339	+·0001891
A_{19}	+·0002133	+·0001224	....
A_{21}	+·0000509	....	....

we shall have

$$H_7 = -4\,a_7 + \frac{91}{16}A_7 + \frac{215}{16}\Delta^2 A_5 + \frac{175}{16}\Delta^4 A_3 + \frac{125}{48}\Delta^6 A_1 = \cdot 04861$$

$$H_9 = -4\,a_9 + \frac{91}{16}A_9 + \frac{215}{16}\Delta^2 A_7 + \frac{175}{16}\Delta^4 A_5 + \frac{125}{48}\Delta^6 A_3 = \cdot 07091$$

$$H_{11} = -4\,a_{11} + \frac{91}{16}A_{11} + \frac{215}{16}\Delta^2 A_9 + \frac{175}{16}\Delta^4 A_7 + \frac{125}{48}\Delta^6 A_5 = \cdot 04469$$

$$H_{13} = -4\,a_{13} + \frac{91}{16}A_{13} + \frac{215}{16}\Delta^2 A_{11} + \frac{175}{16}\Delta^4 A_9 + \frac{125}{48}\Delta^6 A_7 = \cdot 00249$$

$$H_{15} = -4\,a_{15} + \frac{91}{16}A_{15} + \frac{215}{16}\Delta^2 A_{13} + \frac{175}{16}\Delta^4 A_{11} + \frac{125}{48}\Delta^6 A_9 = -\cdot 02230$$

$$H_{17} = -4\,a_{17} + \frac{91}{16}A_{17} + \frac{215}{16}\Delta^2 A_{15} + \frac{175}{16}\Delta^4 A_{13} + \frac{125}{48}\Delta^6 A_{11} = -\cdot 02558$$

$$H_{19} = -4\,a_{19} + \frac{91}{16}A_{19} + \frac{215}{16}\Delta^2 A_{17} + \frac{175}{16}\Delta^4 A_{15} + \frac{125}{48}\Delta^6 A_{13} = -\cdot 01835$$

$$H_{21} = -4\,a_{21} + \frac{91}{16}A_{21} + \frac{215}{16}\Delta^2 A_{19} + \frac{175}{16}\Delta^4 A_{17} + \frac{125}{48}\Delta^6 A_{15} = -\cdot 01023$$

$$H_{23} = -4\,a_{23} + \frac{91}{16}A_{23} + \frac{215}{16}\Delta^2 A_{21} + \frac{175}{16}\Delta^4 A_{19} + \frac{125}{48}\Delta^6 A_{17} = -\cdot 00487.$$

The coefficients of the assumed series being found, and being expressed in seconds of a degree, the part of the refractions depending on Q_3 will be as follows:

$$
\begin{aligned}
f' \times \sin\theta \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \times Q_3 = f' \times \sin\theta \times \Big\{ & e^7 \times 35''\!\cdot\!324, && \log.\ 1\cdot 54807 \\
& + e^9 \times 51\cdot 529, && 1.71205 \\
& + e^{11} \times 32\cdot 476, && 1\cdot 51156 \\
& + e^{13} \times 1\cdot 809, && 0\cdot 25755 \\
& - e^{15} \times 16\cdot 205, && 1\cdot 20965 \\
& - e^{17} \times 18\cdot 588, && 1\cdot 26925
\end{aligned}
$$

$$\begin{array}{lll} & '' & \text{log.} \\ - e^{19} \times & 13{\cdot}334, & 1{\cdot}12498 \\ - e^{21} \times & 7{\cdot}427, & 0{\cdot}87080 \\ - e^{23} \times & 3{\cdot}480, & 0{\cdot}54158 \end{array}\Bigg\}$$

The amount of this expression at the horizon, or when $e = 1$, is $f' \times 62''{\cdot}1$, almost the same with $f' \times 62''{\cdot}4$, which, as is shown in § 9, is the limit of the integral when it is extended from $x = 0$ to $x = \infty$. It is thus proved that the error of the series is of no account. This part of the refraction cannot be computed because f' is unknown. But although the precise value of f' is uncertain, it is probably very considerably less than f, or $\frac{2}{9}$; so that the effect on the refraction cannot exceed a few seconds even at the horizon. We shall be better able to form a just notion with respect to this point, when the Theoretical Table in this paper is compared with observations.

13. It remains to investigate the corrections that must be made in the practical application for the deviations indicated by the meteorological instruments from the mean constants used in constructing the table.

For this purpose we have

$$\delta\,\theta = \sin\theta \times \frac{\alpha\,(1+\alpha)}{\sqrt{5\,i}} \times \mathrm{S},$$

$$\mathrm{S} = \mathrm{Q}_0 + \lambda\,\mathrm{Q}_1 - f\,\mathrm{Q}_2,$$

$$\frac{\sqrt{5\,i}}{\cos\theta} = \frac{e}{1-e^2},$$

$$\lambda = \frac{\alpha}{i}.$$

The quantities e and λ depend only upon α and i: α varies both with the barometer and thermometer, and i, with the thermometer only: the quantity f does not seem liable to change in our climate. Admitting that the prefix d refers only to variations of the barometer and thermometer, we shall have

$$\delta\,\theta + d\,.\,\delta\,\theta = \sin\theta \times \frac{\alpha\,(1+\alpha)}{\sqrt{5\,i}} \times \left\{ \left(1 + \frac{d\,\alpha}{\alpha} - \frac{1}{2}\cdot\frac{d\,i}{i}\right).\,\mathrm{S} \right.$$
$$+ \frac{d\,e}{e}\cdot\frac{d\,\mathrm{S}}{d\,e}\,e$$
$$\left. + \frac{d\,\lambda}{\lambda}\cdot\lambda\,\mathrm{Q}_1 \right\}.$$

Now

$$\frac{d\,e}{e} = \frac{1}{2}\cdot\frac{d\,i}{i}\cdot\frac{1-e^2}{1+e^2},$$

$$\frac{d\,\lambda}{\lambda} = \frac{d\,\alpha}{\alpha} - \frac{d\,i}{i}:$$

wherefore

$$\delta\,\theta + d\,.\,\delta\,\theta = \delta\,\theta\left(1 + \frac{d\,\alpha}{\alpha}\right)$$

$$- \sin\theta \cdot \frac{\alpha(1+\alpha)}{\sqrt{5\,i}} \cdot \frac{d\,i}{i}\left(\frac{S}{2} - \frac{1}{2} \cdot \frac{1-e^2}{1+e^2} \cdot \frac{d\,S}{d\,e}\,e\right)$$

$$+ \sin\theta \cdot \frac{\alpha(1+\alpha)}{\sqrt{5\,i}} \cdot \left(\frac{d\,\alpha}{\alpha} - \frac{d\,i}{i}\right) . \lambda\,Q_1.$$

If p denote the observed height of the barometer, reduced to the fixed temperature of 50° of FAHR.; and τ the temperature of the air on the same scale; then, $\beta = \frac{1}{480}$,

$$1 + \frac{d\,\alpha}{\alpha} = \frac{1}{1+\beta(\tau-50)} \cdot \frac{p}{30},$$

$$\frac{d\,\alpha}{\alpha} = -\frac{\tau-50}{480} - \frac{30-p}{30},$$

$$\frac{d\,i}{i} = +\frac{\tau-50}{480},$$

$$\frac{d\,\alpha}{\alpha} - \frac{d\,i}{i} = -2 \times \frac{\tau-50}{480} - \frac{30-p}{30}.$$

These values being found, if we put

$$T = \sin\theta \times \frac{\alpha(1+\alpha)}{\sqrt{5\,i}} \times \frac{1}{480} \times \left(\frac{(1+e^2)\,S - (1-e^2)\frac{dS}{d\,e}\,e}{2\,(1+e^2)} + 2\,\lambda\,Q_1\right),$$

$$b = \sin\theta \times \frac{\alpha(1+\alpha)}{\sqrt{5\,i}} \times \frac{2\,\lambda\,Q_1}{30};$$

the expression of the mean refraction with its correction will be as follows,

$$\delta\,\theta + d\,.\,\delta\,\theta = \frac{\delta\,\theta}{1-\beta(\tau-50)} \cdot \frac{p}{30} - T\,.\,(\tau - 50) - b\,(30 - p).$$

The first term of this expression is the mean refraction corrected in the manner usually practised by Astronomers. If we assume that the temperature of the mercury in the barometer is the same with that of the air, this term will be equal to

$$\frac{1}{1+\beta(\tau-50)} \cdot \frac{1}{1+\frac{\tau-50}{10000}} \cdot \frac{p}{30} = \frac{1}{1+c\,(\tau-50)} \cdot \frac{p}{30},$$

$$c = \cdot 002183,$$

the new factor being added to compensate the expansion of the mercury. Two subsidiary tables are given for computing this part: Table II. contains the logarithms of $\frac{1}{1+c\,(\tau-50)}$ for 30° on either side of the mean temperature 50°, negative indices being avoided by substituting the arithmetical complements; and Table III. contains the logarithms, or the arithmetical complements, for all values of p from 31 to 28.

The coefficients, T and b, of the other two terms vary with the distance from the

zenith; and they can be computed in no other way than by reducing them to series of the powers of e. By substituting for $\lambda\, Q_1$, the equivalent series already known, we immediately obtain

$$b = \sin\theta \cdot \frac{\alpha(1+\alpha)}{\sqrt{5i}} \cdot \frac{1}{30} \cdot \left\{ B_3\, e_3 + B_5\, e^5 + B_7\, e^7 + \&c. \right\}.$$

Further, by expanding S and its differential, the expression of T will take this form,

$$T = \sin\theta \cdot \frac{\alpha(1+\alpha)}{\sqrt{5i}} \cdot \frac{1}{480} \cdot \left\{ G_3\, e^3 + G_7\, e^7 + G_9\, e^9 + \&c. \right\};$$

and we shall have

$G_3 = A_1 - A_3 + 2\, B_3 = 0{\cdot}2436$

$G_5 = -A_1 + 3\, A_3 - 2\, A_5 + 2\, B_5 = 0{\cdot}4523$

$G_7 = A_1 - 3\, A_3 + 5\, A_5 - 3\, A_7 + 2\, B_7 = 0{\cdot}4705$

$G_9 = -A_1 + 3\, A_3 - 5\, A_5 + 7\, A_7 - 4\, A_9 + 2\, B_9 = 0{\cdot}3502$

$G_{11} = A_1 - 3\, A_3 + 5\, A_5 - 7\, A_7 + 9\, A_9 - 5\, A_{11} + 2\, B_{11} = 0{\cdot}2092$

$G_{13} = -A_1 + 3\, A_3 - 5\, A_5 + 7\, A_7 - 9\, A_9 + 11\, A_{11} - 6\, A_{13} + 2\, B_{13} = 0{\cdot}1050.$

The series for T and b being now known, the coefficients of the terms must next be expressed in seconds of a degree; which being done, the following final results will be obtained.

		log.			log.
$T = \sin\theta \times \{ e^3$	$.\ 0''{\cdot}369,$	$-\ 1{\cdot}5668,$	$b = \sin\theta \times \{ e^3$	$.\ 0''{\cdot}530,$	$-\ 1{\cdot}7240$
$+\ e^5$	$.\ 0{\cdot}685,$	$-\ 1{\cdot}8356$	$+\ e^5$	$.\ 1{\cdot}113,$	$0{\cdot}0465$
$+\ e^7$	$.\ 0{\cdot}712,$	$-\ 1{\cdot}8526$	$+\ e^7$	$.\ 1{\cdot}350,$	$0{\cdot}1306$
$+\ e^9$	$.\ 0{\cdot}530,$	$-\ 1{\cdot}7263$	$+\ e^9$	$.\ 1{\cdot}207,$	$0{\cdot}0817$
$+\ e^{11}$	$.\ 0{\cdot}317,$	$-\ 1{\cdot}5006$	$+\ e^{11}$	$.\ 0{\cdot}873,$	$1{\cdot}9412$
$+\ e^{13}$	$.\ 0{\cdot}159,$	$-\ 1{\cdot}2013$	$+\ e^{13}$	$.\ 0{\cdot}539,$	$-\ 1{\cdot}7313$

The values of T and b are added in separate columns of the annexed table for altitudes less than 10°: for greater altitudes they are omitted as of no account. The application for finding the corrected refraction from the formula

$$\delta\theta + d.\delta\theta = \frac{\delta\theta}{1 + c(\tau - 50)} \cdot \frac{p}{30} - T(\tau - 50) - b(30 - p),$$

will best be explained by the examples afterwards given.

14. The Theoretical Table of refractions which has been computed by the foregoing formulas, and which is deduced solely from the phenomena of the atmosphere without arbitrary assumptions, is next to be compared with the tables most esteemed by astronomers. Two tables more eminently deserve this character; namely, BESSEL's table with its supplement in the *Tabulæ Regiomontanæ*, which may be considered as the result of observations, and as being nearly exact to 88° or 88°½ from the zenith; and

the table published annually in the *Connaissance des Temps*. As all the tables are supposed to contain the same series of refractions, the numbers corresponding to the same altitude should have constantly the same proportion: so that taking the number α which answers to the zenith-distance θ in BESSEL's table, the logarithm of the refraction at the same zenith distance in the New Table should be equal to

$$\log \alpha + \log \tan \theta + \cdot 00507,$$

the number ·00507 being the difference of the logarithms of the refractions at the altitude of 45° in the two tables: but, in the supplemental table, which contains the logarithms of the refractions, it is sufficient to add ·00507 to obtain the logarithms in the New Table. With regard to the refractions in the *Conn. des Temps*, it is more convenient to use the Table in the *Tables Astronomiques* published by the French Board of Longitude: for the logarithms in this table with the addition of ·0011, should agree respectively with the logarithms of the New Table. According to these directions the following comparative view has been drawn up.

Zenith dist.	Refractions.		
	New Table.	Tab. Reg.	Conn. des Temps.
°	"	"	"
10	10·30	10·30	10·30
20	21·26	21·26	21·26
30	33·72	33·72	33·72
40	48·99	48·99	48·99
45	58·36	58·36	58·36
50	69·52	69·52	69·52
55	83·25	83·24	83·25
60	100·85	100·85	100·86
65	124·65	124·62	124·65
70	159·16	159·11	159·22
75	214·70	214·58	214·83
80	320·19	319·88	320·63
81	353·79	353·38	354·33
82	394·68	394·20	395·37
83	445·42	444·86	445·87
84	509·86	509·23	511·22
85	593·96	593·38	595·80
$85\frac{1}{2}$	646·21	647·10	648·34
86	707·43	707·15	710·07
$86\frac{1}{2}$	779·92	777·36	783·07
87	866·76	864·59	870·37
$87\frac{1}{2}$	971·93	972·21	975·89
88	1101·35	1101·40	1105·1
$88\frac{1}{2}$	1262·6	1265·5	1265·0
89	1466·8	1481·8	1464·9
$89\frac{1}{2}$	1729·5	1764·9	1716·4

From this view it appears that the three Tables agree within less than 1″ as far as 80° from the zenith; the New Table is in accordance with BESSEL's, with slight discrepancies, to 88° or $88\frac{1}{2}$° from the zenith; from 80° to 88° of zenith distance the numbers in the French Table exceed those in BESSEL's, the excess being 2″ at 84°, and 4″ at 88°. But when the distance from the zenith is greater than 80°, the accuracy of

the French Table is questionable, both on account of the hypothetical law of the densities, and because the quantity assumed for the horizontal refraction is uncertain.

A few examples are subjoined, as well for explaining the use of the New Table as for affording some indications of its accuracy at low altitudes. The two first instances are taken from the *Tables Astronomiques*, and are likewise published yearly in the *Conn. des Temps*.

EXAMPLE 1.

$\theta = 86^\circ\, 14'\, 42''$

Therm. $8^\circ{\cdot}75$ cent. $= 47^\circ{\cdot}75$ F.

Barom. $0^{m}{\cdot}741 = 29{\cdot}17$ in.

	86° 10′	2·86345
	4 42″	664
		2·87009
	Therm.	·00214
	Barom.	9·98781
Log $\delta\theta$		2·86004
$\delta\theta$		724·5
$-\,{\cdot}25 \times -\,2\tfrac{1}{4}$		$+\,{\cdot}5$
$-\,{\cdot}4 \times {\cdot}8$		$-\,{\cdot}3$
Corrected refraction		12′ 4″·7
Observed refraction		12 4 ·2

EXAMPLE 2.

$\theta = 86^\circ\, 15'\, 20''$

Therm. $8\tfrac{1}{2}^\circ$ cent. $= 46^\circ{\cdot}9$ F.

Barom. $0^{m}{\cdot}766 = 30{\cdot}16$ in.

	86° 10′	2·86345
	5 20″	753
		2·87098
	Therm.	·00276
	Barom.	·00232
Log $\delta\theta$		2·87606
$\delta\theta$		751·7
$-\,{\cdot}25 \times -\,3{\cdot}1$		$+\,{\cdot}8$
$-\,{\cdot}4 \times -\,{\cdot}16$		$+\,{\cdot}6$
Corrected refraction		12′ 33″·1
Observed refraction		12 32 ·5

EXAMPLE 3.

Mean of 42 sub-polar observations of α Lyræ by Dr. BRINKLEY.

Irish Transactions, 1815.

$\theta = 87^\circ\, 42'\, 10''$

Therm. 35°

Barom. 29·5

	87° 40′	3·00522
	2 10″	392
		3·00914
	Therm.	·01444
	Barom.	9·99270
Log $\delta\theta$		3·01628
$\delta\theta$		1038″·2
$-\,{\cdot}6 \times -\,15$		$+\,9{\cdot}0$
$-\,1{\cdot}13 \times \tfrac{1}{2}$		$-\,0{\cdot}6$
Corrected refraction		17′ 26″·6
Observed refraction		17 26 ·5

EXAMPLE 4.

Mean of 10 observations of Capella, from a memoir of M. PLANA.

Acad. de Turin, tom. 32.

$\theta = 88^\circ\, 24'\, 9''{\cdot}7$

Therm. $47^\circ{\cdot}75$

Barom. 29·75

	88° 20′	3·08087
	4 9″·4	847
		3·08934
	Therm.	·00214
	Barom.	9·99607
Log $\delta\theta$		3·08755
$\delta\theta$		1223″·3
$-\,{\cdot}95 \times -\,2\tfrac{1}{4}$		$+\,2{\cdot}1$
$-\,1{\cdot}6 \times {\cdot}27$		$-\,0{\cdot}4$
Corrected refraction		20′ 25″
Observed refraction		20 24 ·3

We may now inquire how far the refractions are likely to be affected by the term which it was found necessary to leave out, because the present state of our know-

ledge of the phenomena of the atmosphere made it impossible to determine the coefficient f' by which it is multiplied. For this purpose the term alluded to, viz.

$$\sin\theta \times f' \times \frac{\alpha(1+\alpha)}{\sqrt{5i}} \times Q_3,$$

which may be shortly denoted by $f' \times \chi(\theta)$, has been computed by means of the equivalent series, for every half degree between 85° and 88°, the results being as follows:

θ	$f' \times \chi(\theta)$
85°	$f' \times 1''{\cdot}5$
$85\frac{1}{2}$	$f' \times 2{\cdot}0$
86	$f' \times 3{\cdot}3$
$86\frac{1}{2}$	$f' \times 4{\cdot}9$
87	$f' \times 7{\cdot}4$
$87\frac{1}{2}$	$f' \times 11{\cdot}2$
88	$f' \times 17{\cdot}0$

From this view it appears that f', although considerably less than f or $\frac{2}{9}$ may still have some influence on the refractions at very low altitudes. The mean refraction in Bessel's Table, and in the New Table, can hardly be supposed to differ 2″ from the true quantity, which would limit f' to be less than $\frac{1}{10}$. It is a matter of some importance to obtain a near value of f': and it is probable that this can be accomplished in no other way but by searching out such values of f and f' as will best represent many good observed refractions at altitudes less than 5°. If such values were found, our knowledge of the decrease of heat in ascending in the atmosphere would be improved, and the measurement of heights by the barometer would be made more perfect.

April 25th, 1838.

Table I.

Mean Refractions for the Temperature 50° Fahrenheit, and the barometric Pressure 30 inches.

Zenith dist.	δθ.	Log δθ.	Diff.	T.	C.	Zenith dist.	δθ.	Log δθ.	Diff.	T.	C.
° 1	′ ″ 0 1·02	0·0085	3012			° 53	′ ″ 1 17·38	1·88863	1577		
2	2·04	0·3097	1763			54	20·24	1·90440	1596		
3	3·06	0·4860	1252			55	23·25	1·92036	1617		
4	4·08	0·6112	974			56	26·41	1·93653	1638		
5	5·11	0·7086	796			57	29·73	1·95291	1664		
6	6·14	0·7882	675			58	33·23	1·96955	1691		
7	7·17	0·8557	587			59	36·93	1·98646	1722		
8	8·21	0·9144	519			60	40·85	2·00368	1756		
9	9·25	0·9663	466			61	45·01	2·02124	1794		
10	10·30	1·0129	424			62	49·44	2·03918	1836		
11	11·35	1·0553	388			63	54·17	2·05754	1881		
12	12·42	1·0941	359			64	59·23	2·07635	1932		
13	13·49	1·1300	334			65	2 4·65	2·09567	1988		
14	14·57	1·1634	313			66	10·48	2·11555	2048		
15	15·65	1·1947	294			67	16·78	2·13603	2116		
16	16·75	1·2241	278			68	23·61	2·15719	2191		
17	17·86	1·2519	265			69	31·04	2·17910	2275		
18	18·98	1·2784	252			70 00	39·16	2·20186	388		
19	20·11	1·3036	241			10	40·59	2·20573	390		
20	21·26	1·3277	230			20	42·04	2·20963	393		
21	22·42	1·3507	222			30	43·52	2·21356	396		
22	23·60	1·3729	215			40	45·02	2·21752	398		
23	24·80	1·3944	207			50	46·53	2·22150	402		
24	26·01	1·4151	201			71 00	48·08	2·22552	404		
25	27·24	1·4352	195			10	49·65	2·22956	407		
26	28·49	1·4547	189			20	51·25	2·23363	410		
27	29·75	1·4736	185			30	52·87	2·23773	413		
28	31·05	1·4921	181			40	54·53	2·24186	417		
29	32·38	1·5102	177			50	56·21	2·24603	419		
30	33·72	1·5279	173			72 00	57·92	2·25022	423		
31	35·09	1·5452	170			10	59·66	2·25445	425		
32	36·49	1·5622	168			20	3 1·43	2·25870	429		
33	37·93	1·5790	164			30	3·23	2·26299	433		
34	39·39	1·5954	162			40	5·06	2·26732	436		
35	40·89	1·6115	160			50	6·93	2·27168	440		
36	42·42	1·6276	159			73 00	8·83	2·27608	443		
37	44·00	1·6435	156			10	10·77	2·28051	447		
38	45·61	1·6591	155			20	12·74	2·28498	450		
39	47·27	1·6746	155			30	14·75	2·28948	454		
40	48·99	1·6901	154			40	16·80	2·29402	458		
41	50·75	1·7055	152			50	18·88	2·29860	462		
42	52·57	1·7207	151			74 00	21·01	2·30322	467		
43	54·43	1·7358	152			10	23·18	2·30789	470		
44	56·35	1·7510	151			20	25·39	2·31259	475		
45	58·36	1·76611	1512			30	27·66	2·31734	479		
46	1 0·43	1·78123	1514			40	29·95	2·32213	483		
47	2·57	1·79637	1518			50	32·30	2·32696	488		
48	4·80	1·81155	1523			75 00	34·70	2·33184	493		
49	7·11	1·82678	1530			10	37·16	2·33677	497		
50	9·52	1·84208	1539			20	39·65	2·34174	502		
51	12·02	1·85747	1551			30	42·21	2·34676	507		
52	14·64	1·87298	1565			40	44·82	2·35183	512		
53	17·38	1·88863				50	47·48	2·35695			

TABLE I. (Continued.)

Zenith dist.	δθ.	Log. δθ.	Diff.	T.	C.	Zenith dist.	δθ.	Log δθ.	Diff.	T.	C.
75° 50	3′ 47″·48	2·35695	517			83° 00	7′ 25″·42	2·64877	833		
76 00	50·21	2·36212	523			10	35·09	2·65810	948		
10	53·00	2·36735	528			20	45·14	2·66758	970		
20	55·85	2·37263	533			30	55·64	2·67728	985		
30	58·76	2·37796	538			40	8 6·55	2·68713	1005		
40	4 1·74	2·38334	545			50	17·95	2·69718	1028		
50	4·79	2·38879	551			84 00	29·86	2·70746	1047	·10	·16
77 00	7·91	2·39430	557			10	42·31	2·71793	1069		
10	11·11	2·39987	563			20	55·33	2·72862	1092		
20	14·39	2·40550	569			30	9 8·96	2·73954	1116		
30	17·74	2·41119	576			40	23·25	2·75070	1140		
40	21·19	2·41695	583			50	38·23	2·76210	1166		
50	24·72	2·42278	589			85 00	53·96	2·77376	1194	·15	·24
78 00	28·33	2·42867	596			10	10 10·52	2·78570	1219		
10	32·04	2·43463	603			20	27·90	2·79789	1248		
20	35·84	2·44066	611			30	46·21	2·81037	1281		
30	39·75	2·44677	618			40	11 5·55	2·82318	1312		
40	43·76	2·44295	626			50	25·90	2·83626	1342		
50	47·88	2·45921	635			86 00	47·43	2·84968	1377	·24	·39
79 00	52·12	2·46556	642			10	12 10·21	2·86345	1412		
10	56·47	2·47198	650			20	34·34	2·87757	1448		
20	5 0·94	2·47848	659			30	59·92	2·89205	1488	·31	·51
30	5·54	2·48507	669			40	13 27·11	2·90693	1627		
40	10·28	2·49176	677			50	55·99	2·92220	1570		
50	15·16	2·49853	688	·03	·04	87 00	14 26·76	2·93790	1612	·39	·67
80 00	20·19	2·50541	696			10	59·54	2·95402	1658	·43	·75
10	25·36	2·51237	707			20	15 34·55	2·97060	1704	·47	·83
20	30·70	2·51944	716			30	16 11·93	2·98764	1758	·52	·91
30	36·20	2·52660	727			40	52·10	3·00522	1808	·58	1·01
40	41·88	2·53387	738			50	17 35·12	3·02330	1862	·63	1·13
50	47·74	2·54125	749	·04	·05	88 00	18 21·35	3·04192	1918	·69	1·24
81 00	53·79	2·54874	761			10	19 11·07	3·06110	1977	·78	1·41
10	6 0·04	2·55635	772			20	20 4·68	3·08087	2040	·87	1·58
20	6·50	2·56407	785			30	21 2·60	3·10127	2102	·96	1·75
30	13·18	2·57192	797			40	22 5·22	3·12229	2169	1·07	2·00
40	20·09	2·57989	811			50	23 13·11	3·14398	2239	1·19	2·24
50	27·26	2·58800	824	·05	·08	89 00	24 26·8	3·16637	2316	1·32	2·48
82 00	34·68	2·59624	838			10	25 46·8	3·18943	2388	1·52	2·91
10	42·37	2·60462	851			20	27 14·2	3·21331	2461	1·72	3·34
20	50·33	2·61313	866			30	28 49·5	3·23792	2529	1·92	3·77
30	58·59	2·62179	883			40	30 33·2	3·26321	2573	2·20	4·34
40	7 7·19	2·63062	899			50	32 15·1	3·28894		2·48	5·00
50	16·13	2·63961	916	·07	·11	90 00	34 32				
83 00	25·42	2·64877									

Table II.

Thermometer.					
	Log.	Diff.		Log.	Diff.
50°	0·00000		50°	0·00000	
49	0·00094		51	9·99906	
48	0·00190		52	9·99811	
47	0·00285		53	9·99717	
46	0·00380		54	9·99623	
45	0·00476	96	55	9·99529	
44	0·00572		56	9·99434	94
43	0·00668		57	9·99341	
42	0·00764		58	9·99248	
41	0·00861		59	9·99154	
40	0·00957		60	9·99061	
39	0·01053		61	9·98969	
38	0·01151	98	62	9·98875	
37	0·01248		63	9·98783	92
36	0·01346		64	9·98690	
35	0·01444		65	9·98598	
34	0·01541		66	9·98506	
33	0·01640		67	9·98414	
32	0·01738		68	9·98323	
31	0·01837		69	9·98231	
30	0·01935		70	9·98140	
29	0·02033		71	9·98049	
28	0·02133	100	72	9·97958	
27	0·02232		73	9·97867	
26	0·02331		74	9·97777	90
25	0·02432		75	9·97686	
24	0·02531		76	9·97596	
23	0·02630		77	9·97506	
22	0·02730		78	9·97416	
21	0·02832	102	79	9·97326	
20	0·02933		80	9·97237	

Table III.

Barometer.		
	Log.	Diff.
In. 31	0·01424	
30·9	0·01248	
8	0·01143	
7	0·01002	142
6	0·00860	
5	0·00718	
4	0·00575	
3	0·00432	144
2	0·00289	
1	0·00145	
30·0	0·00000	
29·9	9·99855	
8	9·99709	
7	9·99563	
6	9·99417	146
5	9·99270	
4	9·99123	
3	9·98075	148
2	9·98826	
1	9·98677	
29·0	9·98528	150
28·9	9·98378	
8	9·98227	
7	9·98076	
6	9·97924	
5	9·97772	152
4	9·97620	
3	9·97466	
2	9·97313	
1	9·97158	
28·0	9·97004	154

X. *Researches on the Tides.—Ninth Series. On the Determination of the Laws of the Tides from short Series of Observations. By the Rev.* W. Whewell, *M.A., F.R.S., Fellow of Trinity College, Cambridge.*

Received June 8th,—Read June 14, 1838.

1. THE discussion of tide observations, for the purpose of obtaining from them the laws of the phenomena, has hitherto been usually conducted upon the supposition that a series of several years in succession was requisite, in order that the accidental irregularities might disappear in the *means* of the observations, and the effects of the lunar inequalities thus come clearly into view. But in the present memoir I shall endeavour to show with what degree of exactness the laws of the phenomena, and the effects of the lunar inequalities, may be deduced from shorter series of observations; for example, from series of one year.

2. I conceive that such an investigation will be of value in several ways. If the principal elements of tide tables for each particular place can be obtained with moderate accuracy from short series of observations properly discussed, the formation of such independent tide tables for different places and times will become far less laborious than it has hitherto been deemed, and may be expected to be far more commonly practised. This will be a great advantage, not only because the tide tables will thus become better, but also because several important questions may thus be settled; for instance, whether, and how far, the laws of tide phenomena change from place to place, and from time to time. These questions we are as yet unable to answer with confidence or with accuracy, although they affect the very foundations of all tide theory, as well as the permanent value of tide tables.

3. But there is another consideration which makes it desirable to compare the results of short series with those of long ones. Without such a comparison we cannot appreciate the *practical* accuracy of our tide tables. If, for instance, the mean of nineteen years of tide observations gave a very exact rule for the effect of lunar parallax, while each single year deviated widely from this rule, it would be clear that the individual observations must be commonly affected by casual irregularities considerably greater than the parallax correction; and therefore the practical accuracy of the tables would be very little improved by introducing into them the parallax correction. I hope to establish, on the contrary, in the following pages, that the general law, and the approximate amount, of the parallax correction, may be traced in the observations of a single year; and thus, that the tables are rendered practically

as well as theoretically better, by such a correction being employed. The same is true, though in a less marked manner, of the correction for lunar declination.

4. It is hardly necessary to remark, as an additional recommendation of the mode of discussing tide observations which this memoir contains, that it leads us, quite as well as any other method, to the results of longer series. When we have obtained the table or the curve of the parallax correction for several single years in succession, the more accurate table or curve which the whole group of years would give is the mean of the single instances. We thus obtain all the accuracy which a longer series of years can supply, while we have, at the same time, the advantage of seeing how much each year contributes to this accuracy.

6. In obtaining the laws of tide phenomena from the observations, different modes of procedure may be adopted, and it still remains to be decided which of these modes is the best. For instance, instead of referring each tide to the moon's transit immediately preceding, we may refer it to the transit one day, or a day and a half, or two days anterior; and it is important to determine which of these modes of reference represents most closely the laws of the phenomena. It is also desirable to ascertain what is the nature and amount of the variations, which these alterations of the epoch of the lunitidal interval introduce into the correction tables for declination and parallax. To settle this point has been one object of the present Researches; and for that purpose I have calculated the correction tables for Bristol, referring the tide to the lunar transit one day, one day and a half, and two days anterior to the transit immediately preceding the tide. Of the results of these different hypotheses I shall hereafter speak.

7. I will add that the present memoir, by clearing up such points as I have noticed, appears to be suited to wind up the series of general researches respecting the tides, which I have, during some years, laid before the Royal Society from time to time. For if methods of discussing tide observations at any place be given, so complete, that the general laws of the corrections may be easily obtained from the observations of a single year, and good tables from a few years, there remains nothing to be done except to apply these methods to good observations, and thus to construct and improve our tide tables for all the most important places; an employment which can easily be carried on by those persons who have performed the calculations on which the present and preceding memoirs are founded. And thus the study of the tides might be pursued, and, to do the subject justice, ought to be pursued, in the same manner as the study of the other provinces of astronomy: that is, constant and careful observations should be made of the phenomena; and, as fast as they are made, should be reduced and discussed at the public expense; so as to test the accuracy of the tables already obtained, and to supply the means of making them still more accurate. In this manner also, any new corrections, and any changes in the elements of the old corrections, would be brought into view as soon as there was evidence of

their existence. Till the subject of the tides is thus treated in a manner worthy of its scientific importance, and of the promise which it now holds forth, it must be considered as offering a blot in that system of the national cultivation of astronomy, of which our public observatories are, in other departments of science, such effective and magnificent examples.

8. The reference of the phenomena of the tides to mechanical principles by rigorous reasoning has not yet been executed; and we can see enough of the difficulties of the subject to believe that it will probably be some time before this problem can be fully solved. In the meanwhile, we have an intermediate type of comparison for the facts, in the equilibrium theory of Bernoulli: for that theory, by modifying the epochs and other elements, may be made to represent in an approximate manner the laws of the phenomena. Nor does it appear to be too much to expect, that it may hereafter be rigorously shown from mechanical principles, that the form of an irregular moving fluid mass, constantly dragged along by certain forces, shall at every instant resemble the form of equilibrium which the forces would produce at some anterior epoch, the anterior epoch being somewhat different for the different features of the fluid form. If such a hydrodynamical proposition could be established, almost all the facts hitherto discovered respecting the tides would be fully explained.

9. The materials of the following researches are the tide observations made at Plymouth in the years 1834, 1835, 1836, and 1837, under the direction of the Dock Master and Assistant Dock Master; and the observations made at Bristol in 1834, 1835, 1836, 1837, by direction of the Dock Committee.

The Plymouth observations were discussed by Mr. Dessiou and Mr. D. Ross of the Hydrographer's Office, whose valuable services were placed at my disposal by the Hydrographer, Captain Beaufort. The Bristol observations were discussed by Mr. Bunt, whom I was enabled to engage in this employment by means of grants voted for that purpose at the Meetings of the British Association for the Advancement of Science which took place in 1836 and 1837.

10. The methods of discussing the observations which were employed were somewhat different for the two places. As that practised at my suggestion by Mr. Bunt for the Bristol tides appears to be the better of the two, I will subjoin his description of the process.

I may previously observe that in all cases we employ the method already described in previous papers: namely, upon a series of parallel ordinates* corresponding to the times of moon's transit, we lay down the successive tides, that is, the heights or the lunitidal intervals, as the one or the other are the subject of examination. This curve is more or less irregular, but for most places the leading feature is the zigzag form which arises from the diurnal inequality†. A curve is drawn by the eye so as

* For the purpose of all these operations it is most convenient to have paper ruled into small squares of inches and twelfths of inches.

† Researches, Seventh Series, Philosophical Transactions, 1837.

to cut off this inequality, leaving equal differences above and below. We may then proceed as follows to find the other inequalities.

"Having laid down the observed intervals and heights, referring both to the *apparent* time of the moon's transit, and drawn through each series of points the dotted line which cuts off the diurnal inequality only, but retains every other, I trace off on a piece of transparent paper, having an axis drawn on it extending from $0^h\ 0^m$ to $12^h\ 0^m$ transit, the successive portions of the dotted line of observation just mentioned, which are included between those hours during the first three months of the year; fitting the tracing paper in its place every time by means of the two extreme points of the axis. I thus obtain six irregular curves, the mean of which is found by drawing across them, at equal distances, twenty-four vertical lines, and finding by my scale, a point in each which is the exact mean of the six intersections. In this manner I get four mean curves on separate pieces of paper, which by repeated combinations are reduced into one, being the mean semimenstrual curve for the year.

"The next step is to reduce this curve to a mean parallax (57′·2) at each hour of transit. For this purpose an arrangement must be made, showing the mean parallax for that year at each of the twelve hours, which will be found to vary from about 56′·9 to 57′·5. Thus the mean parallax for 1836, at the several hours of transit, is

Hour of Transit...	h m 0 30	h m 1 30	h m 2 30	h m 3 30	h m 4 30	h m 5 30	h m 6 30	h m 7 30	h m 8 30	h m 9 30	h m 10 30	h m 11 30
Parallax	57′·5	57′·4	57′·3	57′·1	57′·0	56′·9	56′·9	57′·0	57′·1	57′·2	57′·4	57′·5
Mean of all 57′·2.												

"The parallax table of the preceding year, if already discussed, will be sufficiently near for making the requisite small alteration of the curve to the mean parallax: otherwise an approximate parallax table for the current year must be first made. I then calculate the mean declination, which varies, not only as the parallax, slightly from hour to hour, but also considerably from year to year. The hourly differences (being only about half a degree from the mean) I have disregarded, and I prefer marking on each annual curve the mean declination of that year, to any attempt to reduce the different annual curves to one common declination. Having thus very carefully obtained the mean semimenstrual curve, I cut it out nicely on a piece of thick drawing-paper, and laying the intersections of the vertical hour lines of 0^h and 12^h with the axis, on the corresponding point, on my sheets, I pencil in the mean curves, both of time and of height, and then ink them. The residue, or space between the mean curve and that of observation is next transferred to a straight line below.

"In examining this residue, the first step is to lay on an *approximate* line of parallax. This I take from the parallax correction-curves of the preceding year, if it has been discussed. If not, I make an arrangement for every hour of transit, and for 54′, 55′, 56′, and 59′, 60′, 61′ of parallax (omitting 57′ and 58′), of all the vertical distances, at the successive hours of transit, of the curve from the straight line, adding

to each a constant, to avoid negative quantities. I thus obtain twenty-four parcels, the means of which give an approximate correction for $55' \,..\, 60'$ (or rather $55' \,..\, 59\frac{1}{2}'$) at every hour of transit. From this a first line of parallax is laid down, preparatory to the obtaining of the declination corrections.

"This is done by collecting the measured distances from the parallax line to the curve, into parcels of 0° decl. to 13° decl., and 21° decl. to 28° decl. for every hour of transit, omitting those of 14° to 20° decl., and taking the axis, with the mean declination of the year, as a better representative of the mean declinations. The means of these twenty-four parcels give me a declination correction very near the truth. From this I lay down, on the parallax line, the effect of declination, and thus get an approximate curve of declination combined with parallax. Both this and the former curve of parallax are drawn in with pencil only, being merely used as approximations, whence *corrections* of the first parallax and declination *corrections* are obtained. This is done by an arrangement (as before) of the spaces still remaining between the pencil curve of declination combined with parallax, for hours of transit, and $54' \,..\, 56'$, $59' \,..\, 61'$ parallax, which gives a small additional correction of the parallax corrections first obtained. From this corrected parallax correction I draw in, with ink, the true parallax line, making the requisite alteration in the line of declination combined with parallax, which is then also inked in. Finally, a *correction* of the declination *corrections* being made from this latter line, is considered as giving the true effect of the declinations.

"A further improvement has been introduced into the discussions of 1836, which ought not to be neglected. In former arrangements, e. g. of the twelve parcels of observations with the parallaxes 54′, 55′, 56′, the mean of the *measured distances only* was taken, the mean of the *corresponding parallaxes* having been all assumed = 55′, whereas they differ sometimes half a minute. I have now taken, in all cases, both the mean of the observed quantities and the mean of the parallaxes, or declinations corresponding to that particular parcel; these are afterwards reduced to a common value.

"The above description applies equally to the two processes, for time and for height, which are conducted both in the same manner."

11. By the methods thus described, we obtain curves which represent the semimenstrual inequality and the corrections for lunar parallax and declination, both of the times and of the heights, for any assumed parallaxes and declinations both below and above the mean. By operating in this manner upon the observations of each year separately, we obtain curves for each year; and by combining these for a sufficient number of years, we obtain a mean result, which will possess greater accuracy. From these curves, and especially from the mean of them, we can construct tables of the corrections, which may be used in calculating predictions of the tides, such as are commonly called Tide Tables.

12. But though this is undoubtedly one main use of a discussion of tide observations,

our labours were conducted, as I have already stated, with a peculiar view to several questions respecting the connection of tide phenomena at different times and places; and I will now state the answers to these questions, which our results supply.

I. *To which transit of the moon ought we to refer the tide?*

It has been well shown by Mr. Lubbock that the agreement between the empirical laws of the tides and the equilibrium theory is much improved, by referring the tides to a transit anterior to their occurrence by two or three days. He denotes the successive transits of the moon (at intervals of about twelve hours) by the letters A, B, C, D, E, F; F being the transit which immediately precedes (by about two hours) the high water at London: and he finds that the laws of the tides at London and Liverpool agree best with theory when they are referred to the transit B. The tide which reaches London at two hours after the transit F, was at Plymouth about six hours after the transit E; and as from transit B to transit E is about thirty-eight hours, if we refer the Plymouth tides to transit B, we take a transit about forty-four hours before the tide. Transits A and C are about fifty-six and thirty-two hours anterior to the tide. Nearly the same may be said of Bristol, for the tide there is nearly contemporaneous with that at Plymouth. I have, therefore, referred the Bristol tides of 1834 and 1835 to each of the three transits A, B, C, and have compared the results.

The general result is, that the transit B gives the best tables. This is shown both by the unaccounted-for residue of the observed quantities, which is smallest when transit B is used; and by the form of the curves for the parallax and declination corrections, which is most regular for this transit.

We may therefore, it would seem, assume, at least for our own coasts, that the tides are to be referred to a transit of the moon, which takes place a day and a half before the tide reaches the coasts of Enrope.

We may, however, observe that we do not in this way obtain an exact agreement of observation and theory, even with regard to the semimenstrual inequality. It has appeared from Mr. Lubbock's researches respecting the Liverpool tides*, that while the transit A gives a very exact agreement of the theoretical and observed times, we must take a still earlier transit if we would obtain this agreement with respect to the heights. Nor does that selection of a transit which best represents the semimenstrual inequality, bring about an agreement with theory in the parallax and declination corrections, as we shall see. We must allow, therefore, that though there appear to be, in the actual laws of the tides, inequalities *corresponding* to all these which arise from the supposition of the equilibrium-tide of an anterior epoch transmitted along the ocean to our shores, we cannot so assume the epochs to produce all the inequalities at once. The epoch is of one value for the times, of another for the heights; different again for the parallax correction, and again different for the effect of declination.

* Philosophical Transactions, 1836, Part II.

II. *How does a change of the epoch affect the semimenstrual inequalities?*

The moon's transit takes place every half-day about twenty-four minutes later by solar time. The semimenstrual inequality, both of interval and of height, is referred to the solar time of the moon's transit. The height is the same whether it be referred to the transit E immediately preceding, or to the transit B, which is a day and a half sooner. Also if the moon moved uniformly in her orbit, the *inequality* of the interval of the tide and the transit would be the same, whether the tide were referred to the transit E, or to any anterior one, as B; for the interval is increased by a constant quantity (twelve hours twenty-four minutes) for every transit that we go back. But though the inequality of the interval for any given tide would be the same, it would not occupy the same place in the table or curve, since it would be referred to a different hour of transit. For example, if the mean interval of transit and tide at Bristol, referred to transit E, be 7^h, and if when the moon's transit is 2^h, the tide be at 8^h, the interval is 6^h, and therefore in this case the inequality is *minus* one hour. If now we refer this tide to the transit B, a day and a half sooner, the interval of the transit and tide will be $6^h + 36^h + 72^m$, and the mean interval will be $7^h + 36^h + 72^m$; and therefore, as before, the inequality is *minus* one hour. But the moon's transit E taking place at 2^h, solar time, the transits D, C, B will take place at $1^h\,36^m$, $1^h\,12^m$, $0^h\,48^m$ respectively, and therefore the inequality of minus one hour, which was referred to the transit happening at 2^h when transit E was used, is referred to the transit at $0^h\,48^m$, when we employ the anterior epoch B.

We thus see, that by referring to an anterior epoch, the whole semimenstrual inequality is moved backwards through twenty-four minutes of lunar transit, for every step of one transit backwards. This is the mean result; supposing the moon's motion to be uniform, and neglecting all other inequalities; on which suppositions nothing would be gained or lost in accuracy by the change of epoch. In the actual case we shall find this mean result modified by the influence of the other inequalities, which make one transit a better epoch than another.

The transfer of the semimenstrual inequality curve which we are thus led to expect on changing the epoch, shows itself in the results of the Bristol observations. For example, the time of transit for which the semimenstrual inequality of the interval is 0, being taken for the three transits A, B, C, we find

	For A.	For B.	For C.
In 1834,	$6^h\ 4^m$;	$6^h\ 28^m$;	$6^h\ 52^m$.
In 1835,	$5^h\ 58^m$;	$6^h\ 22^m$;	$7^h\ 4^m$.

Again, for the semimenstrual inequality of heights we find that the minimum height corresponds to the following hours of moon's transit:

	For A.	For B.	For C.
In 1834,	$5^h\ 30^m$;	$5^h\ 54^m$;	$6^h\ 24^m$.
In 1835,	$5^h\ 29^m$;	$5^h\ 46^m$;	$6^h\ 18^m$.

The maximum height corresponds to the following hours of moon's transit:

	For A.	For B.	For C.
In 1834,	$11^h\ 27^m$;	$11^h\ 57^m$;	$12^h\ 28^m$.
In 1835,	$11^h\ 25^m$;	$11^h\ 47^m$;	$12^h\ 7^m$.

We may observe, that if we could find an epoch which should make the results exactly correspond with the theory, the semimenstrual inequality of the time would be 0^h for the time of transit 6^h; and the height would have its maximum and minimum values for the times of transit 6^h and 12^h. From the above results it follows, by an easy calculation, that the epoch which makes the inequality in time agree with the theory is about the transit A; and the epoch which makes the inequality of heights agree with the theory is about four hours after the transit B. We have therefore here still clearer evidence, of that which the discussion of the Liverpool tides by Mr. Lubbock sufficiently showed, that there is no one anterior epoch which will reduce the observed tides to an agreement with the equilibrium theory.

III. *How does a change of the epoch affect the (lunar) parallax correction of the time?*

The parallax corrections of the times, calculated with different epochs, have a general agreement with each other, and with the theory. Taking the correction for parallax 60′ as our example, it has a maximum and a minimum which correspond to transits differing only about three hours from each other; so that the inequality increases from its minimum to its maximum through about nine hours of transit, and then diminishes more rapidly from the maximum to the minimum through three hours. In this all the empirical curves and the theoretical curve agree. But in general the empirical curves differ in two respects from the theoretical one; first, according to theory, the transit which gives the maximum and that which gives the minimum are equally distant from 6^h; one being as much before as the other after that period; whereas in the observation, this mean point corresponds to $6\frac{1}{2}^h$ or 7^h transit; and secondly, the empirical correction contains, besides the variable part, a constant part which depends on the parallax, but does not vary with the hour of transit. This I had already noticed in the second series of these Researches*.

It follows from what is there stated, that while theory gives the formula†

$$(\mathrm{P}-p)\,(b \sin 2\,\phi),$$

observation gives such a formula as

$$(\mathrm{P}-p)\,\left(a+b \sin 2\,(\phi-\beta)\right).$$

But the quantity a as well as β, changes with the epoch, as Mr. Lubbock has justly observed; and the question occurs, whether we can, by a proper assumption of the epoch, cause the quantity a to disappear, and thus render the curve which represents the observed law symmetrical with regard to the axis, as the theoretical curve is.

* Philosophical Transactions, 1834, p. 40.

† Here P is the parallax, p the mean parallax, ϕ the hour of moon's transit, β a time depending on the epoch.

This question our discussion of the Bristol tides enables us to answer. For if we take the parallax correction for 60′, for the year 1834, and for the transit A, we find that it has a maximum 16^m at $5\frac{1}{4}^h$ transit; a minimum $= 1^m$ at $7\frac{3}{4}^h$ transit. Hence it may be represented by $8\frac{1}{2}^m - 7\frac{1}{2}^m \cdot s, 2\phi - 13^h$, where $s, 2\phi$ represents a function of 2ϕ, which differs from $\sin 2\phi$ in having its maximum and minimum at a smaller distance than six hours ($2\frac{1}{2}$ hours in this case), the distance depending on some other quantity*; but which, like $\sin 2\phi$, is 0 when ϕ is 0, + when ϕ is less than 6^h, — when ϕ is greater than 6^h; and has for its maximum and minimum values + 1 and — 1, at equal distances from the value $2\phi = 0$.

In the same manner we may reduce to formulæ of the same kind the parallax corrections for the other transits. And putting them together we have the following results.

		$\quad{}^m \qquad\quad {}^m \qquad\qquad\quad {}^h$
Year 1834.	Transit A.	$8\frac{1}{2} - 7\frac{1}{2} \cdot s, 2\phi - 13$
	Transit B.	$10\frac{3}{4} - 12\frac{1}{4} \cdot s, 2\phi - 14$
	Transit C.	$3\frac{1}{2} - 7\frac{1}{2} \cdot s, 2\phi - 14$
Year 1835.	Transit A.	$9\frac{1}{2} - 7\frac{1}{2} \cdot s, 2\phi - 13\frac{1}{2}$
	Transit B.	$8\frac{1}{2} - 10\frac{1}{2} \cdot s, 2\phi - 14$
	Transit C.	$2\frac{1}{2} - 9\frac{1}{2} \cdot s, 2\phi - 14$

These results show, that by taking a later transit the quantity a is diminished, although irregularly; and therefore the epoch which would reduce the correction to the symmetry of theory is later than C; but we cannot pretend to say with precision how much, without further calculation. We may observe that the circumstance of the coefficients of the variable part being largest for the transit B, appears to indicate that the discussion relative to that transit is the best-conditioned for bringing into view the parallax inequality.

The results of the Bristol observations for 1836 and 1837, discussed for transit B, give, for the parallax correction of the times,

$$\text{for 1836,}\quad 6^m + 9^m \cdot s, 2\phi - 14^h;$$
$$\text{for 1837,}\quad 11^m + 12^m \cdot s, 2\phi - 13^h.$$

The Plymouth observations for 1834, 1835, 1836, 1837, referred to the transit B, give a parallax correction, which follows nearly the same laws as those just stated for Bristol. It has a maximum of 22^m at 5^h, and a minimum of -3^m at 9^h. Hence its formula is (for 61′ parallax)

$$9\tfrac{1}{2}^m - 12\tfrac{1}{2}^m \cdot s, 2\phi - 14^h,$$

which agrees very nearly with the Bristol formula for transit B.

IV. *How does a change of the epoch affect the (lunar) declination correction of the times?*

* The expression $\dfrac{\sin 2(\phi - \beta)}{1 + c\cos 2(\phi - \beta)}$ is an instance of such an expression, the maximum and minimum being equally distant from the value $\phi = \beta$, and each of these distances depending upon the value of c.

Observations similar to those which have been made concerning the correction of the times for parallax, may be repeated with respect to the declination. The theoretical correction is of the form*

$$(\sin^2 \delta - \sin^2 \Delta)\ d\,.\,s,\ 2\,\phi;$$

but observation gives

$$(\sin^2 \delta - \sin^2 \Delta)\ (c + d\,.\,s,\ 2\,\phi - 2\,\beta).$$

And the quantity c varies with a change of the epoch.

The following are the results of the Bristol observations.

For the year 1834, transit A, we find for the declination 7°, a maximum 7^{m} at $4\frac{1}{4}^{h}$; a minimum -7^{m} at $7\frac{3}{4}^{h}$. We find also for the declination 24°, a minimum -3^{m} at 5^{h}; a maximum 9^{m} at $7\frac{1}{4}^{h}$. Hence the corrections for declination of this series may be expressed by these formulæ:

$$\text{for } 7°,\ 0^{m} - 7^{m}\,.\,s,\ 2\,\phi - 12^{h};\ \text{ for } 24°,\ 3^{m} + 6^{m}\,.\,s,\ 2\,\phi - 12\tfrac{1}{4}^{h}.$$

Collecting in the same manner the corrections for the other series, we have

		Decl. 7°.	Decl. 24°.
		m m h	m m h
Year 1834.	Transit A,	$0 - 7\,.\,s,\ 2\,\phi - 12,$	$3 + 6\,.\,s,\ 2\,\phi - 12\frac{1}{4}$
	B,	$-\frac{1}{2} - 5\frac{1}{2}\,.\,s,\ 2\,\phi - 12\frac{1}{2},$	$\frac{1}{2} + 7\frac{1}{2}\,.\,s,\ 2\,\phi - 13$
	C,	$5 - 5\,.\,s,\ 2\,\phi - 14,$	$-2\frac{3}{4} + 5\frac{1}{4}\,.\,s,\ 2\,\phi - 13\frac{3}{4}$
Year 1835.	Transit A,	$1\frac{3}{4} - 8\frac{1}{4}\,.\,s,\ 2\,\phi - 12,$	$2\frac{1}{4} + 5\frac{3}{4}\,.\,s,\ 2\,\phi - 12$
	B,	$1\frac{1}{2} - 7\frac{1}{2}\,.\,s,\ 2\,\phi - 13\frac{1}{4},$	$-1\frac{1}{2} + 7\frac{1}{2}\,.\,s,\ 2\,\phi - 13$
	C,	$7 - 11\,.\,s,\ 2\,\phi - 14,$	$-1 + 7\,.\,s,\ 2\,\phi - 14$

We here see that the transits A and B make c very small, so that when they are used, the declination correction curve approaches very near to the symmetry of theory. Also these epochs make the curve cut the axis very nearly at 6^{h}, as the theory gives.

The observations at Bristol for 1836 and 1837 give for the declination corrections, transit B being used,

	Decl. 7°.	Decl. 24°.
	m m h	m m h
1836,	$1\frac{1}{2} - 7\frac{1}{2}\,.\,s,\ 2\,\phi - 13\frac{3}{4};$	$\frac{1}{2} + 5\frac{1}{2}\,.\,s,\ 2\,\phi - 12;$
1837,	$3\frac{3}{4} - 7\frac{3}{4}\,.\,s,\ 2\,\phi - 12\frac{3}{4};$	$-1\frac{3}{4} + 7\frac{1}{2}\,.\,s,\ 2\,\phi - 12.$

The Plymouth observations for 1834, 1835, 1836, 1837, referred to transit B, give in like manner for the low declination 6°, a curve which has a maximum 10^{m} at 5^{h}, and a minimum -3^{m} at $8\frac{1}{2}^{h}$. Hence its formula would be

$$3\tfrac{1}{2}^{m} - 6\tfrac{1}{2}^{m}\,.\,s,\ 2\,\phi - 13\tfrac{1}{2}^{h}.$$

The correction for 24° has a minimum of -9^{m} at 4^{h}, a maximum of 4^{m} at 8^{h}. Hence its formula is

$$-2\tfrac{1}{2}^{m} + 6\tfrac{1}{2}^{m}\,.\,s,\ 2\,\phi - 12^{h}.$$

V. *How does a change of the epoch affect the parallax correction of heights?*

* Here δ is the declination, and Δ the mean declination.

The parallax correction of heights, according to theory, is of the form

$$(P - p)\,(a + b\,.\,s,\ 2\,\varphi).$$

This agrees with the Bristol observations, which give

$$(P - p)\,(a + b\,.\,s,\ 2\,\varphi - 2\,\beta)\,;$$

for the amount of this correction for a given parallax (as 60′, for example,) is nearly constant for all hours of transit, having, however, a maximum about $6\frac{1}{2}^{h}$ and a minimum about $8\frac{1}{2}^{h}$. Hence β is about $7\frac{1}{2}^{h}$ in this case; the ratio of b to a, which is that of the solar to the lunar tide in theory, appears to be smaller in fact. Indeed for transit A the quantity b vanishes or becomes very small, and the parallax correction is nearly constant for all hours. This appears to show that the transit B is under better conditions than A for obtaining this correction.

The Plymouth parallax correction of heights follows nearly the same law. It has a maximum about $5\frac{1}{2}^{h}$ and a minimum about $8\frac{1}{2}^{h}$.

VI. *How does a change of the epoch affect the declination correction of the heights?*

The declination correction of heights as collected from the Bristol observations is somewhat irregular, the curve for the high and the low declinations being in some cases different in form. It does not appear that the change of epoch very much modifies these forms, at least in any steady manner. For all the epochs (transit A, B, and C) the correction for low declinations (as 7°) has a maximum about $6\frac{1}{2}^{h}$, which diminishes both ways, so as to make a protuberance in the curve from about 4^{h} to 8^{h}, the rest of the correction being nearly constant. For the high declinations (as 24°) the curve, in the best discussions (those of 1836 and 1837), appears to have nearly the same form as for the low declinations.

The Plymouth observations of this correction offer a minimum about 4^{h}, and a maximum about 7^{h}.

VII. *Does the parallax correction of height vary as the parallax?*

In the course of the discussions of the Plymouth tides, the parallax correction of height was taken for each of the twelve hours of transit, and laid down by means of ordinates corresponding to the minutes of transit from 54′ to 61′. The result was, for each year separately (1834–7), very nearly the same straight line, inclined to the axis, and cutting the axis about 57′, which is the mean value of the parallax. The mean of all these years does not differ sensibly from a straight line for each hour; thus showing that the parallax correction of the height does, in reality, vary in proportion to the parallax itself, as according to the theory it ought to do.

We may remark, however, as appears from the same discussion, that from 5^{h} to 9^{h} transit the highest parallaxes are wanting; and that for these hours the mean parallax is smaller than 57′, and the correction line cuts the axis before 57′, whereas for the other hours the point of intersection is after 57′. This result arises in a great measure from the moon's variation, which, increasing her distance from the earth at

quadrature, diminishes the parallaxes in that part of her orbit, and makes their greatest values less than they are in other situations.

The Bristol observations give the same result, which was established by our examination in a manner somewhat different. In 1836 the sum of the parallax corrections of height for all the twelve hours, from the parallax 60′ to the mean 57′·2, was 315 parts; from 57′·2 to 55′ it was 246 parts; the simple ratio of the parallaxes would give 315 to 247·5.

VIII. *Does the parallax correction of time vary as the parallax?*

The observations just referred to enable us to answer this question in the affirmative. In 1836 the corrections of time from 60′ to 57′·2 were 125, and from 57′·2 to 55′ they were 96 parts; the simple ratio of the parallaxes gives 125 to 98·2.

IX. *Does the declination correction of heights vary as the square of the declination?*

According to the theory, the correction for declination, both in the time and the height, will vary as the square of the declination, or as the square of its sine nearly, the correction being supposed to be applied to the declination 0°. And it was shown in the examination of the London tides*, that this is the law which prevails with tolerable regularity in the mean of nineteen years' observations, both for the times and heights. We may perhaps consider this rule therefore as established by much better evidence than two or three years' observations can supply. Still it is not unimportant to examine how far this rule manifests itself in the result of short series of observations. The Plymouth tides were examined with reference to this point. The diurnal, semimenstrual, and parallax corrections being determined and subtracted, the residues of the heights were for each hour expressed by ordinates corresponding to the degrees of declination (taken in groups 0°..3°, 3°..9°, 9°..15°, 15°..21°, 21°..28°). It appeared that in each year (from 1834 to 1837) the curve thus obtained was nearly a straight line, and it was still more nearly so in the mean of the years. This result appeared to show that the declination correction of heights is proportional to the declination itself, and not to its square. But the Bristol observations give a different result. Thus in 1835 the correction in height (for all the twelve hours together) from decl. 7°·3 to mean decl. 18°·6 was 192 parts; and from 18°·6 to 24°·4 it was 173 parts. This proportion of 192 to 173 is very nearly agreeable to the proportion of the squares of the declinations; indeed it is a still higher ratio; for the proportion of the squares gives 192 to 164, while the simple ratio of the declination would give 192 to 98. In 1836 the correction in height from decl. 8° to the mean 19°·7 was 227 parts; and from 19°·7 to 25° it was 138. The law of the squares would require the proportion 227 to 166, the law of the simple ratio, 227 to 103. In this case the result is nearly intermediate between the square and the simple ratio, but the mean of the two years gives the law of the square of the declination very nearly. And this result, agreeing with the London mean of nineteen years, may be considered as sufficiently established;

* Researches, Second Series, Philosophical Transactions, 1834, pp. 24. 32.

the occasional deviations in short series arising from the irregularity of the phenomena, and from the imperfection of the other corrections.

X. *Does the declination correction of times vary as the square of the declination?*

The Bristol observations give

1835. Total time correction from decl. $7^\circ{\cdot}3$ to $18^\circ{\cdot}6$; 81 parts
from — $18^\circ{\cdot}6$ to $24^\circ{\cdot}4$; 75 —

The ratio of the squares is 81 to 69; the simple ratio is 81 to 41.

1836. Total time correction from decl. 8° to $19^\circ{\cdot}7$; 76 parts
from — $19^\circ{\cdot}7$ to 25; 46 —

The ratio of the squares is 76 to 56; the simple ratio is 76 to 34. In both these cases the actual result is in a ratio considerably higher than the simple ratio; and the mean approaches near to the ratio of the square.

The Plymouth tides of 1835, 1836, 1837, were discussed in a different manner, the declination corrections for each hour being kept separate. The effects due to declination being thrown into curves according to the degrees of declination, the form of these curves from 2^h to 6^h transit appeared to show very clearly, that at those hours the correction is as the square of the declination; while for the other hours of transit the correction is small and irregular. It has already been shown that the declination correction of times is of the form $(\sin^2 \delta - \sin^2 \Delta)\ (c + d\,.\,s,\ 2\,\phi - 2\,\beta)$, and the form of the irregularity appears to indicate that β is smaller for the high than for the low declinations. It may perhaps be possible hereafter to put this correction in a form which shall escape this irregularity.

XI. *Can the laws of the corrections be deduced from a single year?*

Whether the observations of a single year, or of a few years, suffice for determining the tide corrections, is a question which the discussions just described enable us to answer. For if the curves representing the laws of these corrections be regular for each year and similar in successive years, we cannot doubt that the law is given with some approximation by each, although the mean may be still more accurate. On examining our curves with this view we find that most of the corrections are given with tolerable precision, even by a single year's observations. The semimenstrual inequality may be determined from that period, and gives a very regular curve if the observations are good. It may be still improved by reducing each hour to the parallax and declination which is the mean of the whole, as I shall further explain. The parallax correction from a single year is less regular, as might be expected, the observations for each hour of transit being further distributed to each minute of parallax, so that each resulting number is the mean of a few observations only. Still the general form of the parallax correction curve for one year exhibits the features which belong to the mean; namely, a maximum and minimum about 4^h and 8^h both for time and height. And when the process is conducted with care, and the curves which represent the corrections adjusted nearer and nearer to the observations, in

the manner which the discussion itself suggests, the results may be rendered far more exact than would have been conceived possible. This was done by Mr. Bunt in the discussions of the Bristol tides for 1836 and 1837, and the result is that the form of the parallax correction deduced from the observations above, and from those below the mean, is almost identical. Without using any arbitrary improvement of the curves, they are absolutely symmetrical above and below the axis. The same is the case with the curves which express the corrections for high and for low declinations. The above methods applied to a single year appear to give as good forms of the correction curves as were at first obtained from the whole mass of nineteen years' observations. It is clearly shown by our researches, that a series of five years will give, with great regularity, the laws and amounts of all the corrections, especially if we attend to what we now know of their general form, and of the cycles of longer period by which they are affected.

XII. *Are there any regular differences between the corrections of successive years?*

The mean parallax of one year is very nearly the same as the mean parallax of another year*. But the case is different with the declination. The inclination of the moon's orbit continues nearly the same during one year, but varies from year to year in consequence of the revolution of the nodes. When the ascending node of the moon's orbit is in the first point of Aries, the inclination of the orbit to the equator is the sum of its inclination to the ecliptic, and of the obliquity of the ecliptic. When the descending node is at the first point of Aries, the inclination to the equator is the difference of the other two. In this manner the inclination of the moon's orbit to the equator may vary from about $18\frac{1}{2}°$, which value it had in 1829 and 1830, to $28\frac{1}{2}°$, which is its value in the present year 1838. Consequently the mean declination of the moon for different years will be different, and the semimenstrual curve obtained by taking the mean of the year will correspond to different declinations in different years. The mean declination for 1834, obtained by taking the correction to be proportional to the square, is, for 1834, $17°{\cdot}2$; for 1835, it is $18°{\cdot}6$; for 1836 it is $19°{\cdot}7$; for 1837 it is $20°{\cdot}2$. Hence on this account alone the semimenstrual curve of 1837 and 1834 would differ. For instance, for 8^h transit, the difference of effect of declination $17°{\cdot}2$ and $20°{\cdot}2$ is 6^m or 8^m; and hence the semimenstrual curves will deviate from each other by such a quantity. Accordingly this feature appears in the results of the Bristol observations; and we have in this fact a very remarkable evidence that the effects of declination are discoverable in the tides of each year.

The Bristol observations for 1834, 1835, 1836, and 1837 being reduced, according to this view, to a common parallax and declination, give the following results, from which the agreement of different years may be judged of.

* The mean parallax of the different hours of transit is not the same: the greatest parallax occurring near the syzygies, in consequence of the moon's variation, as I have already observed. Hence the semimenstrual curve obtained by taking the mean of each hour of transit, requires a correction for parallax to reduce it to the general mean.

Hour of Transit	h m 0 30	h m 1 30	h m 2 30	h m 3 30	h m 4 30	h m 5 30	h m 6 30	h m 7 30	h m 8 30	h m 9 30	h m 10 30	h m 11 30
Times. Semimenstrual Inequality. Mean Parallax 57′·2. Mean Declination 19°.												
Interval.	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m
1834. (Mean Interval 43^h 56^m)	44 3	43 47	43 30	43 15	43 8	43 21	43 57	44 34	44 44	44 42	44 34	44 19
1835. (Mean Interval 43^h 36^m)	44 8	43 48	43 29	43 13	43 7	43 21	44 2	44 34	44 44	44 42	44 33	44 21
1836. (Mean Interval 43^h 56^m)	44 4	43 45	43 28	43 15	43 6	43 19	44 4	44 36	44 47	44 44	44 35	44 22
1837. (Mean Interval 43^h 56^m)	44 7	43 47	43 29	43 13	43 5	43 20	44 2	44 35	44 47	44 45	44 37	44 24
Mean of 1834–7	44 5·5	43 46·7	43 29	43 14	43 6·5	43 20·2	44 1·2	44 34·7	44 45·5	44 43·2	44 34·7	44 21·5
Greatest Difference	2·5	1·7	1·0	1·0	1·5	1·2	4·2	1·3	1·5	1·8	2·3	2·5
Heights. Semimenstrual Inequality. Mean Parallax 57′·2. Mean Declination 19°.												
	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.
1834.	31·6	31·0	29·3	26·9	24·3	22·1	22·3	25·0	27·6	29·6	31·0	31·7
1835.	31·7	30·7	28·8	26·5	24·0	22·2	22·8	25·3	27·8	29·7	31·1	31·7
1836.	31·5	30·7	29·1	26·8	24·1	22·0	22·5	25·0	27·6	29·6	31·0	31·6
1837.	31·7	30·7	29·0	26·8	24·2	22·0	22·3	24·8	27·4	29·8	31·2	31·8
Mean of 1834–7	31·6	30·8	29·0	26·8	24·1	22·1	22·4	25·0	27·6	29·7	31·1	31·7
Greatest Difference	0·14	0·25	0·22	0·21	0·14	0·09	0·32	0·03	0·20	0·11	0·10	0·13

It appears from these Tables that the greatest difference of the semimenstrual inequality of time in any one year from the mean of the four years is $4\frac{1}{5}$ minutes; and the greatest difference of height in the same manner is about 4 inches. It is to be recollected that this is the error in a mean tide of 33 feet.

XIII. *Do the corrections at different places agree in laws and amount?*

The amount of agreement in this respect will be seen by the following statement.

The semimenstrual inequality in time, although according to theory it is determined by the proportion of the force of the moon to that of the sun, is by the observations different at different places. The total amount of this inequality, that is, the difference of the greatest and least lunitidal intervals, is 95 minutes at Plymouth, and 93 minutes at Bristol. But these are greater than the values which this inequality assumes at any other places. It is 90^m at London and at Sheerness; 86^m at Liverpool and Howth; 84^m at Leith; 83^m at Portsmouth and at Pembroke; 82^m at Ramsgate; and only 80^m at Brest. In each of these cases the value is determined from observations so numerous as to be certain within a minute or two. We see, therefore, how different the mass of the moon would be found to be by calculations from the tide observations of different places.

The comparison of the semimenstrual inequality of height at different places gives a similar result. In this case, however, we must not take the actual amount of the inequality, but its proportion to the mean tide. At Portsmouth the mean range of the tide is $12\frac{1}{2}$ feet; the total semimenstrual inequality, or difference of the height of high water at neap and spring tides, is $2\frac{1}{2}$ feet; that is, only $\frac{1}{5}$ of the mean tide. But at Plymouth, where the mean tide is also $12\frac{1}{2}$ feet, the total semimenstrual inequality is 3·4 feet, and thus the fraction is $\frac{1}{3\cdot4}$. And at Bristol, where the mean range of tide is 33 feet, the semimenstrual inequality is 10 feet, or $\frac{1}{3\cdot3}$ of the mean tide.

The parallax corrections of time are as follows (for 60′), in minutes and decimals:

London . . . . $- \; 4^{m}\cdot6 - 8^{m}\cdot8 \,.\, s, 2\,\phi - 14^{h}$;
Liverpool . . . $2\cdot5 - 7\cdot3 \,.\, s, 2\,\phi - 13$;
Plymouth . . . $7\cdot2 - 9\cdot1 \,.\, s, 2\,\phi - 14$;
Bristol, 1834, $10\cdot7 - 12\cdot3 \,.\, s, 2\,\phi - 14$;
1835, $8\cdot5 - 10\cdot5 \,.\, s, 2\,\phi - 14$;
1836, $6 - 9\cdot \,.\, s, 2\,\phi - 13$;
1837, $11 - 12\cdot \,.\, s, 2\,\phi - 13$.

The parallax corrections of height are as follows, in decimals of feet (for 60′):

London . . . . $\cdot75 + \cdot23 \,.\, s, 2\,\phi - 18^{h}$;
Liverpool . . . $1\cdot35 + \cdot09 \,.\, s, 2\,\phi - 18$;
Plymouth . . . $\cdot57 + \cdot09 \,.\, s, 2\,\phi - 14$;
Bristol, 1834, $2\cdot35 + \cdot65 \,.\, s, 2\,\phi - 15$;
1835, $2\cdot1 + \cdot60 \,.\, s, 2\,\phi - 16$;
1836, $1\cdot85 + \cdot65 \,.\, s, 2\,\phi - 14$;
1837, $2\cdot1 + \cdot50 \,.\, s, 2\,\phi - 13\tfrac{1}{2}$.

The constant part, which is here much the most considerable, is nearly in the proportion of the mean tide.

The declination corrections of time are (for 7°),

Plymouth . . . $3^{m}\cdot5 - 6^{m}\cdot5 \,.\, s, 2\,\phi - 12\tfrac{1}{2}^{h}$;
Bristol, 1834, $- \; 0\cdot5 - 5\cdot5 \,.\, s, 2\,\phi - 12\tfrac{1}{2}$;
1835, $1\cdot5 - 7\cdot5 \,.\, s, 2\,\phi - 13\tfrac{1}{4}$;
1836, $1\cdot5 - 7\cdot5 \,.\, s, 2\,\phi - 13\tfrac{3}{4}$;
1837, $3\cdot3 - 7\cdot7 \,.\, s, 2\,\phi - 12\tfrac{3}{4}$.

The declination corrections of height are (for 7°),

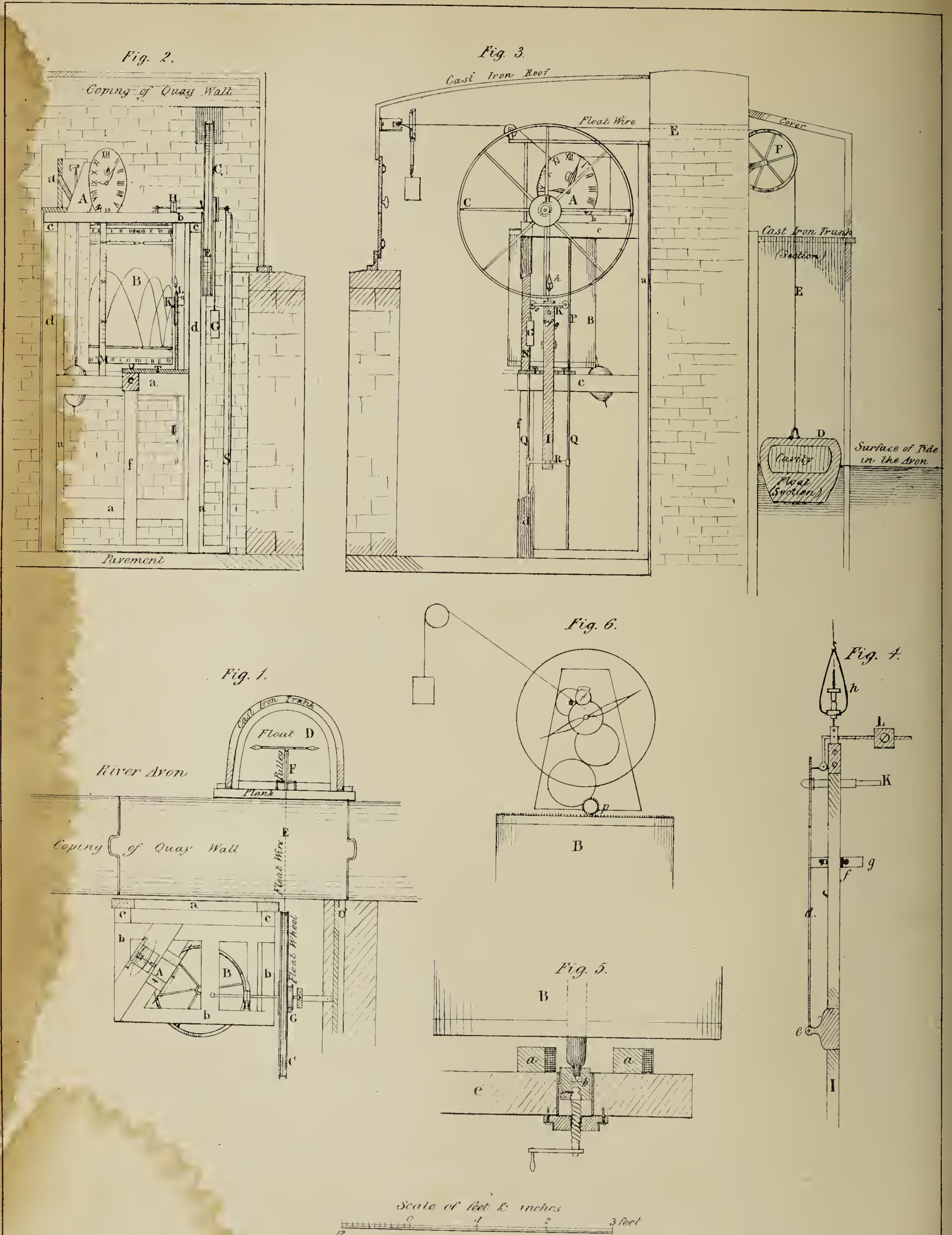
Plymouth . . . $\cdot28 + \cdot15 \,.\, s, 2\,\phi - 9^{h}$;
Bristol, 1834, $\cdot9 + \cdot6 \,.\, s, 2\,\phi - 14$;
1835, $1\cdot1 + \cdot3 \,.\, s, 2\,\phi - 15\tfrac{1}{4}$;
1836, $1\cdot5 + \cdot7 \,.\, s, 2\,\phi - 15\tfrac{3}{4}$;
1837, $1\cdot3 + \cdot5 \,.\, s, 2\,\phi - 15\tfrac{1}{2}$.

I do not compare with these the declination corrections for London and Liverpool, because they are calculated from a different mean declination (15°), and therefore a difference might not prove a real discrepancy. For the same reason the Bristol results for different years are not to be expected to agree exactly.

The mean declination of the moon, about which the effects balance each other in an entire revolution of her nodes, appears by calculation to be about $16\tfrac{1}{2}°$. It is this declination which should, in the Tables, have the correction 0; and the semimenstrual inequality of all years must be taken for this mean declination, in order to

make the correction tables general. I have not yet reduced the Bristol corrections to this basis, having a hope of still further improving the declination correction; and also of obtaining a solar correction from the observations. These tasks, however, must be reserved for a future occasion, if I am able to execute them at all. I hope the present attempt to solve some of the most obvious and important questions of Tidology will not be without its value.

Trinity College, Cambridge,
June 5, 1838.



J. Basire, lith.

XI. *Description of a new Tide-Gauge, constructed by Mr.* T. G. Bunt, *and erected on the eastern bank of the river Avon, in front of the Hotwell House, Bristol*, 1837. *Communicated by the Rev.* William Whewell, *M.A. F.R.S. &c.*

Received March 15,—Read March 22, 1838.

THE principal parts of this machine are:

A, an eight-day clock, which turns

B, a vertical cylinder, revolving once in twenty-four hours:

C, a wheel, to which an alternate motion is communicated by

D, a float rising and falling with the tide, and connected with C by the wire E, passing over the pulley F, and kept constantly strained by the counterpoise G:

H, a small drum on the same axis with C, which, by a suspending wire, communicates one-eighteenth of the vertical motion of the float to

I, a bar carrying a pencil, K, which marks a curve on the cylinder B, so as to show the time and height of high water.

The above will convey a general notion of the machine and its use; but it will be necessary to enter more into detail.

a (figs. 1, 2 and 3) is a strong oak frame secured to the quay wall; b is a mahogany frame resting on the arms c c, which are supported by the pillars d d; e is a bracket resting on a pillar f, and sustaining the cylinder B. The lower pivot of the cylinder has its bearing in a small cylindrical plug *b* (fig. 5.), which may be lowered by a screw and a winch, as seen in the figure, so as to disengage, first the upper pivot, and afterwards the lower one, when the cylinder may be drawn out of its place, being made to slide forwards horizontally on the two cross pieces *a a*.

The pencil K, fig. 4, is inserted into a tube soldered to the brass bar *d*. This bar moves without shake, on a long axis *e*, so as not to require side friction to guide it, while it is pressed forwards against the cylinder by the weight and bent lever L; *f* is a small bolt for holding back the pencil, when necessary, by thrusting it into the stud *g*; *h* is a nut and screw for adjusting the height of the pencil to the scale. The pencil is guided vertically by a mahogany bar N, fig. 3, having a groove in its edge for receiving the ivory wheel *o*, which is pressed gently against N by the brass rod P, slightly curved towards the pencil, to produce the requisite pressure. Q Q are two other brass rods (or tubes) for guiding the lower end of the pencil bar I, by means of the cross guide R, fitted loosely on the rods. The guide N is adjusted, or may be drawn out, by means of a notch in the board T, which supports it, the top of the bar being held by a tenon. The pillar d is represented as broken off, for the purpose of

showing the guides N and Q. The outer support S, fig. 2, of the axis of the wheel C, is also removed.

The cylinder, which is 24 inches in length and 48 in circumference, is composed of mahogany staves 2 inches in width and 1 inch in thickness. It has two heads and a diaphragm, also of mahogany, into which every stave is screwed by a screw at each end, and another in the middle. A strong iron axis passes through the whole length of the cylinder, which revolves on a rounded steel pivot screwed into the lower end of the axis. The surface is a white enamel, fit for receiving the mark of a pencil. On the top is screwed a brass wheel, equal in diameter to the cylinder, and divided into 360 teeth. A two-hour pinion (p) of thirty teeth, at the bottom of the clock face, causes this wheel to perform one revolution in twenty-four hours. This pinion, together with the rest of the train, is seen in fig. 6.

The clock is of a superior kind, and larger size than ordinary, having a fir pendulum rod and a dead-beat escapement. It goes exceedingly well, and appears to suffer no derangement in the regularity of its motion from the incumbrance of the cylinder. This, however, is very trifling, a weight of half an ounce being sufficient to overcome it. The pallets may be detached from the train by lifting a latch behind the clock, and drawing them backwards. This arrangement is required from its being necessary to *fix* the hands so that they cannot, as in other clocks, be made to slide without carrying the train along with them. The pinion which drives the cylinder may be detached from it by pulling it forwards, so as to permit the cylinder to be turned freely round.

Although the workmanship of the clock is good, yet, from inequalities in the teeth of the wheels, the cylinder is not moved round through exactly equal spaces in equal times. To prevent error from this cause, the hours and minutes marked round the top of the cylinder, were divided out in the following manner. The cylinder being connected with the clock, but the pallets and pendulum detached from it, the hands, which now move rapidly forward, are brought to the instant of $0^h\ 0^m$, where they are held fast while a vertical line is drawn on the cylinder, by the pencil moving between the guides. The hands are then moved forwards 20 minutes at a time, and a line drawn on the cylinder at each of these periods, until the whole circumference has been divided into 72 spaces. These are afterwards subdivided to every 5 minutes. Another necessary precaution is, to *mark* a tooth of the large wheel on the cylinder, and the *two corresponding teeth* of the small wheel which turns it, and in the same manner to mark all the wheels in the train, so that whenever the clock is taken to pieces to be cleaned or repaired, the wheels may be put together again, with the teeth in exactly the same relative positions as before. By this means, a source of considerable error is entirely obviated.

The scale of height may be obtained, by marking the outside of the trunk containing the float into feet, and watching the ascent of the surface of the tide until it reaches one of the lower marks, when a mark must be instantly made with the pencil

on the cylinder. Successive marks, as the tide and pencil continue to ascend, at length complete the scale. A better and more expeditious method is by accurately measuring and dividing the circumference of the large float wheel, and then marking the cylinder with the pencil as it is wound up, by moving the wheel round and bringing its divisions successively up to a fixed index.

The pencil curves may be either received on the enamelled surface of the cylinder, or they may be taken off on paper, very correctly, in the following manner. The sheet having been wrapped round the cylinder, and expanded by rubbing it all over with a wet sponge, is then pasted together at the ends, and secured in its place, either with drawing pins, or with elastic bands, as shown in the drawing; after which it soon becomes quite tight, and the curves may be taken and read off on the paper with nearly as much accuracy as on the enamel itself, with the additional advantage of their being preserved.

The times and heights are read off by means of the vertical scale M, which is held at the two extremities by notches cut in the frame, and may be pushed into contact with the cylinder. The large wheel being detached from the clock, and a short chord-line drawn under the summit of each of the curves and bisected, the point of bisection is turned round to the edge of the scale, the upper end of which shows the time of high water, amongst the divisions of hours and minutes on the cylinder. The height is read off by noticing where the summit of the curve falls amongst the graduations of the scale. Thus in fig. 2. the curve in contact with the scale indicates the time of high water to be $0^h\ 40^m$ P.M. and the height 32 feet 11 inches.

The float is of pine, well saturated with oil, and has hitherto preserved sufficient buoyancy. It is without friction rollers, which it is better to dispense with when its motion is very nearly vertical. The trunk is of cast iron $\frac{7}{8}$ths of an inch in thickness, being in a situation where considerable strength was required. At the back of the trunk is a stout wooden plank, and both together are firmly secured with nuts, and bolts inserted with lead into the quay-wall. The joints are caulked, and the water enters through a moveable shutter in the bottom, by an aperture of about $\frac{1}{200}$th of the sectional area of the trunk. This was found after many trials to be the best size for preventing unsteady motion of the float, which is now almost entirely removed; a rapid undulation being communicated to it when the aperture was made much larger, and a slow but very considerable oscillation when it was much less.

XII. *Remarks on the Theory of the Dispersion of Light, as connected with Polarization. By the Rev.* BADEN POWELL, *M.A. F.R.S. F.G.S., Savilian Professor of Geometry in the University of Oxford.*

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Introductory Observations.

(1.) IN the course of four successive papers, I have laid before the Royal Society the comparison of observations of the refractive indices for the standard rays in various media, with the results calculated from the formula of theory, as deduced upon the most improved views of the hypothesis of undulations; the cases discussed including the greatest range of data which experiment has yet furnished.

The degree of accordance thus exhibited between observation and theory, even in the most extreme case, I believe will now be considered sufficient to warrant the conclusion, that the theory at least affords a very satisfactory approximation to the expression and explanation of the actual law of nature; and this in the instance of a class of phenomena which it had long been the reproach of that theory to be supposed incapable of accounting for, if they were not absolutely contradictory to it. At any rate, if, in the instances referred to, the discrepancies should by any be thought still too great, or if it should be contended that other cases may yet arise in which theory may be put to a severer test, yet, with so strong a presumption in its favour, the only fair inference would be, that further examination was required of the principles on which any extension or modification of the theory might be pursued.

(2.) The investigation, then, having advanced thus far, it seemed desirable, as a sequel to my former papers, to devote the present to some remarks connected with the *theory* which has been thus applied.

The facts of interference, on which the undulatory theory was originally based by Dr. YOUNG, obliged us to adopt *some* idea of an *alternating* motion, as well as a motion of *translation*, in our conception of light. And this, with all the accessions it has received, especially from the investigations of FRESNEL, has at the present day been connected by the labours of M. CAUCHY and others with general dynamical principles, which regulate the propagation of vibratory motions through an elastic medium.

(3.) More precisely, from such dynamical principles certain differential equations of motion have been deduced; the integration of which gives the well-known expression for a wave, involving the relation between the velocity and the wave-length which explains the dispersion. The *direct* and *complete* integration of these forms, effected

by M. Cauchy, and simplified by Mr. Tovey and Mr. Kelland, involves certain conditions, viz. *the evanescence of certain terms*; the interpretation of which implies peculiar views of the constitution of the ether.

Whether a *direct* and *complete* solution, *without* the introduction of these conditions, can be attained, does not appear. But Mr. Tovey has been the first (as far as I am aware) to point out (in one of his valuable series of papers in the London and Edinburgh Journal of Science) that, *without* these conditions, a certain form of the wave-function *is a particular solution* of the equations; and this form is precisely that expressing *elliptically polarized light.*

(4.) Upon a careful examination of that most important and ingenious investigation, it appeared to me that there were one or two questions connected with the subject still requiring to be cleared up.

If the *absence* of the conditions in question be *essential* to the case of elliptically and circularly polarized light, it follows that *all the preceding investigations* (which depend on the fulfilment of those conditions) *are applicable only to unpolarized and plane-polarized light,* and consequently the *general* integration is limited in a most material part of its application; a defect which is only remedied by the supplementary investigation of Mr. Tovey, in which, for this case, a *particular* solution is assigned.

It seemed, then, necessary to show explicitly that the non-fulfilment of the conditions, that is, the *non-evanescence* of the terms in question, is *essential* for elliptically polarized light, as their *evanescence* is for common light; and thus to exhibit distinctly the relation between the cases of elliptically polarized, and those of plane-polarized and unpolarized light: and again, to remove, if possible, the obscurity and discrepancy of opinion in which the physical interpretation of those conditions, with regard to the supposed constitution of the etherial medium, appeared to be involved.

With this object, it seemed necessary to take up the subject from its first principles; and, in pursuing the inquiry, I gladly acknowledge the valuable suggestions I have derived in the course of some correspondence with Mr. Lubbock. I have been thus led to combine the elucidation of the points just referred to, in a connected view with preceding results, by a simplified method; and to trace clearly the relation between the changes in the conditions supposed with regard to the ether, the resulting state of polarization in the ray, and the expression for the dispersion.

Analytical Investigation.

(5.) For the purpose of the ensuing investigation it will be necessary to premise a brief statement of the formulas adopted from the undulatory theory. On the ordinary principles of that theory, the form

$$u = \Sigma \{\alpha \sin (n\,t - k\,x)\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

is the sum of a number of functions, each expressing a single wave, whose aggregate constitutes a ray of light; (u) is the displacement of the vibrating molecule of ether;

(α) the amplitude of vibration; (t) the time from the beginning of the disturbance; (x) the distance along the ray from the molecule first agitated; while $\left(\frac{n}{k}\right)$ is the velocity of propagation, which further involves the relation

$$\frac{1}{\mu} = \frac{n}{k} \quad \text{and } k = \frac{2\pi}{\lambda}, \quad \ldots \ldots \quad (2.)$$

where (μ) is the refractive index, and (λ) the wave-length in the medium.

(6.) The investigations of FRESNEL require us to suppose the vibrations performed in planes at right angles to the direction of the ray: or, in general, referring the whole motions to three rectangular axes x, y, z, of which we may suppose x to coincide with the direction of the ray, and naming the displacements in each of those directions respectively ξ, η, ζ, we must suppose $\xi = 0$, while each of the others are functions of x and t of the form above, viz.

$$\left.\begin{aligned} \eta &= \Sigma\{\alpha \sin(n\,t - k\,x)\} \\ \zeta &= \Sigma\{\beta \sin(n\,t - k\,x)\} \end{aligned}\right\} \quad \ldots \ldots \quad (3.)$$

In polarized light the amplitudes α β are in FRESNEL'S notation

$$\alpha = \cos i \quad \text{and } \beta = \sin i,$$

where i is the angle formed by the direction of the vibration with that of polarization; whence also

$$\alpha^2 + \beta^2 = 1. \quad \ldots \ldots \quad (4.)$$

In plane polarized light either $i = 0$, or $i = \frac{\pi}{2}$; hence one of the expressions (3.) disappears.

For elliptically polarized light both are retained; but one vibration is retarded by a quantity (b), or we must take

$$\left.\begin{aligned} \eta &= \Sigma\{\alpha \sin(n\,t - k\,x)\} \\ \zeta &= \Sigma\{\beta \sin(n\,t - k\,x + b)\}, \end{aligned}\right\} \quad \ldots \ldots \quad (5.)$$

expressions which will easily be found to give, on substitution of the value of η in that of ζ, the equation to the ellipse described by the etherial molecules, which becomes that of a circle if $b = \frac{\pi}{2}$ and $\alpha = \beta$. If $\beta = 0$ the formulas are reduced to those for common light, and α and β are not restricted by the condition (4.).

(7.) From (2.) we have $\frac{1}{\mu} = \frac{n}{2\pi}\lambda$; $\quad \ldots \ldots \quad$ (6.)

hence we may observe; 1st. If $\frac{n}{k}$ be constant there is no dispersion. In this case it is easily found that the formulas (3.) are solutions of the well-known equation of vibratory motion,

$$\frac{d^2 u}{d\,t^2} = -\frac{n^2}{k^2}\frac{d^2 u}{d\,x^2}.$$

2ndly. If n be an independent constant these expressions give the index in the simple inverse ratio of the wave-length, which is manifestly not the law of the unequal refrangibility of the primary rays, as appears at once from what is termed the irrationality of dispersion. But the application of the formulas to the ordinary phenomena is independent of this, and will be equally valid if μ be dependent on λ, precisely or very nearly in the relation expressed by the formula which has been compared with experiment in my previous papers, and which thus explains the unequal refrangibility.

Now it has been shown by the distinguished mathematicians already mentioned, that, under certain conditions, the expressions (3.) can be derived from equations of motion for the vibrations of an elastic medium on dynamical principles, which also involve the desired relation between μ and λ. It will be necessary to state and explain these equations, which may be most shortly done as follows:

(8.) Let the coordinates in space of any molecules $m\ m'$ be respectively

$$\left.\begin{array}{llll} m \ldots\ldots & x & y & z \\ m' \ldots\ldots & x+\Delta x & y+\Delta y & z+\Delta z \end{array}\right\} \quad \ldots\ldots\ldots \quad (7.)$$

and the distance between them

$$r = \sqrt{(\Delta x^2 + \Delta y^2 + \Delta z^2)} \quad \ldots\ldots\ldots \quad (8.)$$

Let the force which maintains the system of molecules as an elastic medium be any function of the distance as $f(r)$: then it will be easily seen that we have for the forces in the direction of the three axes, in *equilibrium*,

$$\left.\begin{array}{l} \Sigma f(r)\dfrac{\Delta x}{r} = 0 \\ \Sigma f(r)\dfrac{\Delta y}{r} = 0 \\ \Sigma f(r)\dfrac{\Delta z}{r} = 0 \end{array}\right\} \quad \ldots\ldots\ldots \quad (9.)$$

Let the system be *disturbed*, and after a time (t) let the displacements in the direction of the three axes be respectively

$$\left.\begin{array}{llll} m \ldots\ldots & \xi & \eta & \zeta \\ m' \ldots\ldots & \xi+\Delta\xi & \eta+\Delta\eta & \zeta+\Delta\zeta \end{array}\right\} \quad \ldots\ldots\ldots \quad (10.)$$

also the distance becomes

$$r + \Delta r = \sqrt{\{(\Delta x + \Delta\xi)^2 + (\Delta y + \Delta\eta)^2 + (\Delta z + \Delta\zeta)^2\}} \quad \ldots\ldots \quad (11.)$$

In this condition it will be easily seen that we have for the forces in the direction of the three axes,

$$\left.\begin{array}{l} \dfrac{d^2\xi}{dt^2} = \Sigma\left\{f(r+\Delta r)\dfrac{\Delta x + \Delta\xi}{r+\Delta r}\right\} \\ \dfrac{d^2\eta}{dt^2} = \Sigma\left\{f(r+\Delta r)\dfrac{\Delta y + \Delta\eta}{r+\Delta r}\right\} \\ \dfrac{d^2\zeta}{dt^2} = \Sigma\left\{f(r+\Delta r)\dfrac{\Delta r + \Delta\zeta}{r+\Delta r}\right\} \end{array}\right\} \quad \ldots\ldots\ldots \quad (12.)$$

On expanding the value of $\frac{f(r+\Delta r)}{r+\Delta r}$, neglecting the squares, substituting in the above equations (12.), observing that they will involve the expressions (9.) for equilibrium, which vanish, and writing for abridgement,

$$\frac{f(r+\Delta r)}{r+\Delta r} = \varphi(r) = \frac{f(r)}{r} + \psi(r)\, r\, \Delta r.$$

while we have

$$r\, \Delta r = \Delta x\, \Delta \xi + \Delta y\, \Delta \eta + \Delta z\, \Delta \zeta;$$

these equations of motion (12.) will ultimately become

$$\left.\begin{aligned}
\frac{d^2\xi}{dt^2} &= \Sigma\left\{\varphi(r)\,\Delta\xi + \psi(r)\,\Delta x\,(\Delta x\,\Delta\xi + \Delta y\,\Delta\eta + \Delta z\,\Delta\zeta)\right\} \\
\frac{d^2\eta}{dt^2} &= \Sigma\left\{\varphi(r)\,\Delta\eta + \psi(r)\,\Delta y\,(\Delta x\,\Delta\xi + \Delta y\,\Delta\eta + \Delta z\,\Delta\zeta)\right\} \\
\frac{d^2\zeta}{dt^2} &= \Sigma\left\{\varphi(r)\,\Delta\zeta + \psi(r)\,\Delta z\,(\Delta x\,\Delta\xi + \Delta y\,\Delta\eta + \Delta z\,\Delta\zeta)\right\}.
\end{aligned}\right\} \quad . \quad (13.)$$

These equations form the common basis of all the investigations of the subject as originally pursued by MM. NAVIER and CAUCHY. They apply generally to all elastic media, or systems of molecules, affected only by their mutual attractions and repulsions, and slightly disturbed.

(9.) In applying this hypothesis to the case of *light*, agreeably to what has been already observed (§. 6.), we have

$$\Delta\xi = 0 \quad \text{and} \quad \frac{d^2\xi}{dt^2} = 0,$$

so that the equations (13.) are reduced to

$$\left.\begin{aligned}
\frac{d^2\eta}{dt^2} &= \Sigma\left\{\varphi(r)\,\Delta\eta + \psi(r)\,\Delta y\,[\Delta y\,\Delta\eta + \Delta z\,\Delta\zeta]\right\} \\
\frac{d^2\zeta}{dt^2} &= \Sigma\left\{\varphi(r)\,\Delta\zeta + \psi(r)\,\Delta z\,[\Delta y\,\Delta\eta + \Delta z\,\Delta\zeta]\right\}
\end{aligned}\right\} \quad . \; . \; . \; . \quad (14.)$$

The *general direct* integration of these forms, and even of the preceding (13.), has been effected by the writers already referred to; *but only upon the suppositions*

$$\Sigma\{\Delta y\, \Delta z\} = 0, \quad \text{and } \Sigma\{\sin k\, \Delta x\} = 0. \; . \; . \; . \; . \; . \; . \quad (15.)$$

the import of which will be considered in the sequel. Mr. TOVEY by a skilful analysis gives a *particular solution* corresponding to the case of elliptically polarized light, *without introducing those conditions.* The investigation, however, appears susceptible of being simplified; and we shall here pursue a method at once attaining this object, and enabling us more clearly to trace out some interesting results, as follows:

Taking the finite differences of the expressions (5.), we have

$$\Delta\eta = \alpha\left[2\sin^2\left(\frac{k\,\Delta x}{2}\right)\sin(n\,t - k\,x) - \sin k\,\Delta x \cos(n\,t - k\,x)\right] \; . \; . \quad (16.)$$

$$\Delta \zeta = \left\{ \begin{array}{l} \beta \cos b \left[2 \sin^2 \left(\frac{k \Delta x}{2} \right) \sin (n t - k x) - \sin k \Delta x \cos (n t - k x) \right] \\ + \beta \sin b \left[\sin k \Delta x \sin (n t - k x) - 2 \sin^2 \left(\frac{k \Delta x}{2} \right) \cos (n t - k x) \right]. \end{array} \right\} \quad (17.)$$

Also, differentiating them, we find,

$$\frac{d^2 \eta}{d t^2} = - n^2 \alpha \sin (n t - k x) \quad \ldots \ldots \quad (18.)$$

$$\frac{d^2 \zeta}{d t^2} = - n^2 \beta \cos b \sin (n t - k x) - n^2 \beta \sin b \cos (n t - k x) \quad \ldots \quad (19.)$$

On arranging the terms in (14.) we may write for abridgement,

$$\left. \begin{array}{l} p = \varphi (r) + \psi (r) \Delta y^2 \\ p' = \varphi (r) + \psi (r) \Delta z^2 \\ q = \psi (r) \Delta y \Delta z \\ 2 \theta = k \Delta x \end{array} \right\} \quad \ldots \ldots \quad (20.)$$

and those forms (14.) will thus become,

$$\frac{d^2 \eta}{d t^2} = \Sigma \left[p \Delta \eta + q \Delta \zeta \right] \quad \ldots \ldots \quad (21.)$$

$$\frac{d^2 \zeta}{d t^2} = \Sigma \left[p' \Delta \zeta + q \Delta \eta \right] \quad \ldots \ldots \quad (22.)$$

On substituting in these forms the values of $\Delta \eta$ and $\Delta \zeta$ above, viz. (16.) (17.), we shall have,

$$\frac{d^2 \eta}{d t^2} = \Sigma \left\{ \begin{array}{l} \left. \begin{array}{l} q \{ \beta \sin b \} \sin 2 \theta \\ \left. \begin{array}{l} + q \{ \beta \cos b \\ + p \{ \alpha \end{array} \right\} 2 \sin^2 \theta \end{array} \right\} \sin (n t - k x) \\ \left. \begin{array}{l} \left. \begin{array}{l} - p \{ \alpha \\ - q \{ \beta \cos b \end{array} \right\} \sin 2 \theta \\ - q \{ \beta \sin b \} \, 2 \sin^2 \theta \end{array} \right\} \cos (n t - k x) \end{array} \right\} \quad \ldots \quad (23.)$$

$$\frac{d^2 \zeta}{d t^2} = \Sigma \left\{ \begin{array}{l} \left. \begin{array}{l} p' \{ \beta \sin b \} \sin 2 \theta \\ \left. \begin{array}{l} + p' \{ \beta \cos b \\ + q \{ \alpha \end{array} \right\} 2 \sin^2 \theta \end{array} \right\} \sin (n t - k x) \\ \left. \begin{array}{l} \left. \begin{array}{l} - q \{ \alpha \\ - p' \{ \beta \cos b \end{array} \right\} \sin 2 \theta \\ - p' \{ \beta \sin b \} 2 \sin^2 \theta \end{array} \right\} \cos (n t - k x) \end{array} \right\} \quad \ldots \quad (24.)$$

On comparing these expressions with those for the same functions (18.) (19.) which must be identical, and equating in each the terms which form the coefficients respectively of $\sin (n t - k x)$ and of $\cos (n t - k x)$ (since they must hold good for all

values of those variable terms), we shall have the following equations :

$$-n^2\alpha=\Sigma\left\{\begin{array}{ll}+q\{\beta\sin b\} & \sin 2\theta\\ \left.\begin{array}{l}+q\{\beta\cos b\\ +p\{\alpha\end{array}\right\} & 2\sin^2\theta\end{array}\right\}\quad\ldots\ldots\quad(25.)$$

$$0=\Sigma\left\{\begin{array}{ll}\left.\begin{array}{l}-p\{\alpha\\ -q\{\beta\cos b\end{array}\right\} & \sin 2\theta\\ -q\{\beta\sin b\} & 2\sin^2\theta\end{array}\right\}.\quad\ldots\ldots\quad(26.)$$

$$-n^2\beta\cos b=\Sigma\left\{\begin{array}{ll}+p'\{\beta\sin b\} & \sin 2\theta\\ \left.\begin{array}{l}+p'\{\beta\cos b\\ +q\{\alpha\end{array}\right\} & 2\sin^2\theta\end{array}\right\}.\quad\ldots\ldots\quad(27.)$$

$$-n^2\beta\sin b=\Sigma\left\{\begin{array}{ll}\left.\begin{array}{l}-q\{\alpha\\ -p'\{\beta\cos b\end{array}\right\} & \sin 2\theta\\ -p'\{\beta\sin b\} & 2\sin^2\theta\end{array}\right\}\quad\ldots\ldots\quad(28.)$$

From the two last forms (27.) (28.) we obtain by multiplication and addition,

$$-n^2\beta=\Sigma\left\{\begin{array}{ll}\left.\begin{array}{l}p'\{\beta\cos 2b\\ q\{\alpha\cos b\end{array}\right\} & 2\sin^2\theta\\ -q\{\alpha\sin b\} & \sin 2\theta\end{array}\right\}\quad\ldots\ldots\quad(29.)$$

And in like manner from (25.) and (29.) we deduce

$$-n^2(\alpha^2+\beta^2)=\Sigma\left\{\left.\begin{array}{l}p'\{\beta^2\cos 2b\\ +p\{\alpha^2\\ +q\{2\alpha\beta\cos b\end{array}\right\}2\sin^2\theta\right\}\quad\ldots\ldots\quad(30.)$$

In the case of elliptically polarized light we have

$$\alpha^2+\beta^2=1\,;$$

and it is thus obvious that *when the quantities involved give the value of* n^2 *as here expressed*, viz.

$$-n^2=\Sigma\left\{\left.\begin{array}{l}p'\{\beta^2\cos 2b\}\\ +p\{\alpha^2\}\\ +q\{2\alpha\beta\cos b\}\end{array}\right\}2\sin^2\theta\right\}\quad\ldots\ldots\quad(31.)$$

and when this value is substituted in (18.) (19.), *and consequently in* (6.), *the formula* (5.) *for elliptically polarized light is a particular solution of the differential equations of motion of the system of molecules* (14.).

Deductions and Remarks.

(10.) It is evident that in (31.) expressing the whole coefficient by a single letter, we have the abridged form

$$n^2=\Sigma\{H'^2\sin^2\theta\},\quad\ldots\ldots\ldots\quad(32.)$$

which, on dividing by k^2, restoring the value of θ (20.) and of k (2.), will give for the velocity $\left(\text{or}\,\frac{n}{k}\right)$ (on multiplying both numerator and denominator by $\left(\frac{\Delta x}{2}\right)$ on the second side of the equation, and including all the coefficients in a new single term,) the formula

$$\frac{1}{\mu^2} = \Sigma \left\{ \mathrm{H}^2 \frac{\sin^2\left(\frac{\pi \Delta x}{\lambda}\right)}{\left(\frac{\pi \Delta x}{\lambda}\right)^2} \right\}, \quad \ldots \ldots \ldots \ldots \quad (33.)$$

the same which has been deduced for unpolarized and plane polarized light.

From this formula, on the supposition of a common coefficient, I made my approximate computations in Nos. I. and II. of my former Researches. On expanding the sine and dividing by the arc there results a series of even powers of λ, which, in a generalized form, is that from which Sir W. R. Hamilton deduced his elegant formula for interpolation, by which I performed my calculations in No. III. From a similar series, also, M. Cauchy has deduced the elaborate method of calculation which he has applied to Fraunhofer's indices in his *Nouveaux Exercices de Mathématiques,* and also in his lithographed memoir on Interpolation: whilst the same series obtained by Mr. Kelland, and modified by the introduction of the value of λ *in vacuo* (that term being in the above formula the wave-length *in the medium*), or

$$\mathrm{P} = \frac{1}{\mu^2} + \left(\frac{\mu}{\lambda}\right)^2 \mathrm{Q} - \left(\frac{\mu}{\lambda}\right)^4 \mathrm{L} \quad \ldots \ldots \ldots \quad (34.)$$

is the formula which I employed and explained in No. IV. Precisely equivalent results are also deduced by Mr. Tovey.

(11.) The term on which the dispersion depends, viz.

$$\frac{\sin\left(\frac{\pi \Delta x}{\lambda}\right)}{\left(\frac{\pi \Delta x}{\lambda}\right)}, \quad \ldots \ldots \ldots \ldots \quad (35.)$$

manifestly approaches to unity as Δx diminishes in comparison with λ, the application of which was originally pointed out by Mr. Airy. The dispersion is insensible in free space, and large in dense media. Hence Mr. Kelland has inferred that the ether is in a state of *greater* density in free space, and *less* in dense media. We shall recur to this point in the sequel.

(12.) Experiment does not show that the state of a ray as to polarization, produces any difference in the magnitude of its refractive index. Hence it follows that in the formula (30.) *the values of n^2 must be constant for all values of b, as well as of α and β.* Hence for any particular value of b the several sums of terms involved must be supposed to vary in magnitude, so that, joined with the condition (4.), the whole expression shall always remain constant, and equal to that in the case where b vanishes, and where α and β are no longer restricted to the condition (4.).

The examination of the cases thus arising, is closely connected with the question of the evanescence of the terms before alluded to (15.). These relations therefore we shall now proceed to trace. But it will be desirable first to point out more clearly the nature and meaning of those conditions.

(13.) It is evident from the value of r (8.) that it does not change its sign along with a change of sign in Δx, Δy, or Δz; the same is true therefore of $\varphi(r)$ and $\psi(r)$. It is also evident that Δy^2, Δz^2, do not change the signs. But the product $\Delta y \, \Delta z$, will change signs as each value of Δy corresponds to a negative and a positive value of Δz.

Hence the terms

$$\psi(r) \sin k \Delta x$$
$$\psi(r) \Delta y^2 \sin k \Delta x$$
$$\psi(r) \Delta z^2 \sin k \Delta x$$
$$\psi(r) \Delta y \, \Delta z \sin k \Delta x$$
$$\psi(r) \Delta y \, \Delta z \sin^2 \left(\frac{k \Delta x}{2}\right)$$

will have values with opposite signs. But,

$$\psi(r) \sin^2 \left(\frac{k \Delta x}{2}\right)$$
$$\psi(r) \Delta y^2 \sin^2 \left(\frac{k \Delta x}{2}\right)$$
$$\psi(r) \Delta z^2 \sin^2 \left(\frac{k \Delta x}{2}\right)$$

will have all their values with the same sign. If then we consider the sums of a number of such terms respectively, those of the first set may become $= 0$ if the positive and negative sums be equal. In those of the second set this cannot happen.

Hence resuming our abridged notation we may have either

$$\left.\begin{array}{l} \Sigma (p \sin 2\theta) = 0 \\ \Sigma (p' \sin 2\theta) = 0 \\ \Sigma (q \sin 2\theta) = 0 \end{array}\right\} \quad . \quad . \quad . \quad (36.) \qquad \text{or} \qquad \left.\begin{array}{l} \Sigma (p \sin 2\theta) = s \\ \Sigma (p' \sin 2\theta) = s_{\prime} \\ \Sigma (q \sin 2\theta) = s_{\prime\prime} \end{array}\right\} \quad . \quad . \quad . \quad . \quad (38.)$$

$$\Sigma (q \sin^2 \theta) = 0 \quad . \quad . \quad . \quad . \quad (37.) \qquad \Sigma (q \sin^2 \theta) = s_{\prime\prime\prime} \quad . \quad . \quad . \quad . \quad (39.)$$

but always

$$\left.\begin{array}{l} \Sigma (p \sin^2 \theta) = s' \\ \Sigma (p' \sin^2 \theta) = s'' \end{array}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (40.)$$

The conditions which give these values respectively, are dependent on the general supposition relative to the arrangement of the molecules. If the distribution of the molecules in space be uniform, the values (36.) (37.) obtain. If not uniform, the

values (38.) (39.) may obtain; but in particular cases, or for particular directions of the ray, the greater number of terms of one sign may compensate for the greater magnitudes of those of the other; or the conditions (36.) (37.) may hold good.

(14.) We now resume the consideration of the formulas before deduced.

From (28.) we have

$$\sin b = \frac{\alpha \Sigma (q \sin 2\theta) + \beta \cos b \Sigma (p' \sin 2\theta)}{n^2 \beta - 2\beta \Sigma (p' \sin^2 \theta)} \quad \ldots \ldots \quad (41.)$$

Now if the conditions (36.) obtain, that is, if we have

$$\Sigma (q \sin 2\theta) = 0, \text{ and } \Sigma (p' \sin 2\theta) = 0; \quad \ldots \ldots \quad (42.)$$

then (41.) will give

$$\sin b = 0; \text{ or } b = 0. \quad \ldots \ldots \quad (43.)$$

But if the polarization be elliptical, b must have a finite value; or in this case, consequently, the conditions (38.) must obtain; or *the non-evanescence of those terms is essential to the investigation for elliptically polarized light.*

(15.) If the polarization be *circular,* we have

$$\alpha = \beta, \text{ and } b = \frac{\pi}{2} \cos b = 0, \sin b = 1;$$

thus the form (26.) becomes

$$0 = \left\{ \begin{array}{c} \Sigma (p \sin 2\theta) \\ -2\Sigma (q \sin^2 \theta). \end{array} \right\} \quad \ldots \ldots \quad (45.)$$

But the condition of *the non-evanescence of the terms holds good* for the same reason as in the last case. . . . . (46.)

(16.) For plane polarized light let $\beta = 0$, then the form (26.) becomes

$$0 = \Sigma (p \sin 2\theta); \quad \ldots \ldots \quad (47.)$$

or for plane polarization it is essential to suppose the terms evanescent, or the conditions (36.) (37.) *to be fulfilled.* . . . . (48.)

(17.) For unpolarized light we have

$$b = 0 \quad \sin b = 0 \quad \cos b = 1;$$

but α and β are arbitrary. Thus the formula (26.) becomes

$$0 = \left\{ \begin{array}{c} \alpha \Sigma (p \sin 2\theta) \\ +\beta \Sigma (q \sin 2\theta). \end{array} \right\} \quad \ldots \ldots \quad (49.)$$

But since this must be true for all values of α and β which are independent of p, q and θ, it follows that each term separately must be $= 0$; *or for unpolarized light it is essential to suppose the terms evanescent, or the conditions* (36.) (37.) *to be fulfilled.* . . . . (50.)

(18.) We here take the axis (x) as the direction of the ray, which consequently may

have any position whatever in respect to particular lines of direction which may subsist among the molecules of the etherial medium.

Now, supposing the arrangement of the molecules to be perfectly uniform, since the direction of the ray (x) must pass through m, it follows that the sums of the distances of all those on each side of the line x, whether in the plane of y or of z, will be equal, or the hypothesis (36.) (37.), or (48.) (50.) exactly fulfilled, *for all positions of* x in the medium.

If the arrangement be *not* uniform, the same hypothesis in general may be *approximately* fulfilled in proportion as we suppose a *greater number of such molecules taken into account*, or the summation extended to a greater number of terms, that is, a greater number of molecules *within the sphere of the influence of the propagation of motion* from the molecule first agitated. But if we suppose any portion of the ether be so circumstanced that the sphere of influence is more limited, then the conditions (36.) (37.) will not be fulfilled even approximately, or the hypothesis (38.) (39.) will obtain; though they would approach more or less towards fulfilment in different positions of the ray. The diminution of the sphere of influence may arise either from a decrease in the force, or an increase in the distances of the molecules.

(19.) Here also we may remark, in connexion with what was observed before (§. 11.), that at the bounding surface of vacuum and a medium, or generally of two media of different densities, we can hardly suppose the change of density in the ether to take place abruptly; but must, from all analogy, imagine a thin stratum on either side, within which there is a gradual alteration in the arrangement of the molecules, and this more considerable in proportion to the difference of refractive powers of the media.

Thus, even supposing the molecules uniformly distributed in the two media or portions of space, it is evident that within this stratum they will not be uniformly arranged. And hence, though on either side the conditions (36.) (37.) should be fulfilled, *within the stratum* the conditions (38.) (39.) would take effect.

Conclusion.

(20.) The general conclusions which I conceive have been obtained are as follows:

When light is elliptically or circularly polarized, that is, when one of the two component vibrations is retarded behind the other, then, in the differential equations of motion, the opposite terms do not destroy each other in the summation; that is, the arrangement of the molecules is not uniform.

When light is plane-polarized, or unpolarized, that is, when there is no retardation, or the phases of the component vibrations are simultaneous, then the opposite sums destroy each other; that is, either the arrangement is uniform, or the sphere of the influence of the force so great, that the conditions are fulfilled very nearly.

Since both kinds of light can be propagated indifferently through ordinary media, it follows that the sphere of influence of the force, or number of molecules taken into

account, does not here depend on the arrangement of the molecules of ether in the medium, but on the retardation of one of the vibrations behind the other, or the absence of it, originally impressed on the ray in the respective cases.

These inferences appear of importance in connexion with the consideration of the causes on which polarization depends, and the laws of elasticity of the ether in crystallized bodies, as also the state of the ether at the bounding surfaces of media of different density, and the changes which may be effected in the state of polarization of a ray: and in what has here been laid down, I trust that the whole subject (without entering into specific controversy) will be found relieved from some degree of difficulty and objection.

Note to Art. 6.

If in equation (5.) $b = \frac{\pi}{2}$ (without supposing $\alpha = \beta$) the vibration resulting is still elliptical. In this case the expressions may be put into the form

$$\left.\begin{aligned} \eta &= \Sigma \{\alpha \sin (n t - k x)\} \\ \zeta &= \Sigma \{\beta \cos (n t - k x)\} \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (52.)$$

This form is that adopted by Professor Macculagh* to express the elliptical vibrations of the two rays in quartz, and by means of which he connects the laws of M. Biot, and the theory of Mr. Airy, with certain differential equations of vibratory motion, provided the quantities α, β, n, and k, are so assumed as to fulfil these conditions, viz. that if A and B be the squares of the velocities of the ordinary and extraordinary rays in common double refraction, in quartz we replace them respectively by

$$A - k \frac{\beta}{\alpha} C \quad \text{and } B - k \frac{\alpha}{\beta} C$$

where C is a new constant determined from Biot's observations. The differential equations referred to are these:

$$\left.\begin{aligned} \frac{d^2 \eta}{d t^2} &= A \frac{d^2 \eta}{d x^2} + C \frac{d^3 \zeta}{d x^3} \\ \frac{d^2 \zeta}{d t^2} &= B \frac{d^2 \zeta}{d x^2} - C \frac{d^3 \eta}{d x^3} \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (53.)$$

But on taking the partial differential coefficients of the expressions (52.) it is easily seen (from the particular forms of those functions) that the equations (53.) are reducible to,

$$\left.\begin{aligned} \frac{d^2 \eta}{d t^2} &= \left[A - k \frac{\beta}{\alpha} C\right] \frac{d^2 \eta}{d x^2} \\ \frac{d^2 \zeta}{d t^2} &= \left[B + k \frac{\alpha}{\beta} C\right] \frac{d^2 \zeta}{d x^2} \end{aligned}\right\}$$

which are of the well-known form (§. 7.) for vibratory motion.

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XIII. *Experimental Researches in Electricity.—Fourteenth Series.*

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§. 20. *Nature of the electric force or forces.* §. 21. *Relation of the electric and magnetic forces.* §. 22. *Note on electrical excitation.*

§. 20. *Nature of the electric force or forces.*

1667. THE theory of induction set forth and illustrated in the three preceding series of experimental researches does not assume anything new as to the nature of the electric force or forces, but only as to their distribution. The effects may depend upon the association of one electric fluid with the particles of matter, as in the theory of Franklin, Epinus, Cavendish, and Mossotti; or they may depend upon the association of two electric fluids, as in the theory of Dufay and Poisson; or they may not depend upon anything which can properly be called the electric fluid, but on vibrations or other affections of the matter in which they appear. The theory is unaffected by such differences in the mode of viewing the nature of the forces; and though it professes to perform the important office of stating *how* the powers are arranged (at least in inductive phenomena), it does not, as far as I can yet perceive, supply a single experiment which can be considered as a distinguishing test of the truth of these various views.

1668. But, to ascertain how the forces are arranged, to trace them in their various relations to the particles of matter, to determine their general laws, and also the specific differences which occur under these laws, is as important as, if not more so than, to know whether the forces reside in a fluid or not; and with the hope of assisting in this research, I shall offer some further developments, theoretical and experimental, of the conditions under which I suppose the particles of matter are placed when exhibiting inductive phenomena.

1669. The theory assumes that all the *particles*, whether of insulating or conducting matter, are as wholes conductors.

1670. That not being polar in their normal state, they can become so by the influence of neighbouring charged particles, the polar state being developed at the instant, exactly as in an insulated conducting *mass* consisting of many particles.

1671. That the particles when polarized are in a forced state, and tend to return to their normal or natural condition.

1672. That being as wholes conductors, they can readily be charged, either *bodily* or *polarly*.

1673. That particles which being contiguous are in the line of inductive action can communicate or transfer their polar forces one to another *more or less* readily.

1674. That those doing so less readily require the polar forces to be raised to a higher degree before this transference or communication takes place.

1675. That the ready communication of forces between contiguous particles constitutes conduction, and the difficult communication insulation; conductors and insulators being bodies whose particles naturally possess the property of communicating their respective forces easily or with difficulty, and bodies having these differences just as they have differences of any other natural property.

1676. That ordinary induction is the effect resulting from the action of matter charged with excited or free electricity upon insulating matter, tending to produce in it an equal amount of the contrary state.

1677. That it can do this only by polarizing the particles contiguous to it, which perform the same office to the next, and these again to those beyond; and that thus the action is propagated from the excited body to the next conducting mass, and there renders the contrary force evident in consequence of the effect of communication which supervenes in the conducting mass upon the polarization of the particles of that body (1675.).

1678. That therefore induction can only take place through insulators; that induction is insulation, it being the necessary consequence of the state of the particles and the mode in which the influence of electrical forces is transferred or transmitted across such insulating media.

1679. The particles of an insulating dielectric whilst under induction may be compared to a series of small magnetic needles, or more correctly still to a series of small insulated conductors. If the space round a charged globe were filled with a mixture of an insulating dielectric, as oil of turpentine or air, and small globular conductors, as shot, the latter being at a little distance from each other so as to be insulated, then these would in their condition and action exactly resemble what I consider to be the condition and action of the particles of the insulating dielectric itself. If the globe were charged, these little conductors would all be polar; if the globe were discharged, they would all return to their normal state, to be polarized again upon the recharging of the globe. The state developed by induction through such particles on a mass of conducting matter at a distance would be of the contrary kind, and exactly equal in amount to the force in the inductric globe. There would be a lateral diffusion of force (1224. 1297.), because each polarized sphere would be in an active or tense re-

lation to all those contiguous to it, just as one magnet can affect two or more magnetic needles near it, and these again a still greater number beyond them. Hence would result the production of curved lines of inductive force if the inducteous body in such a mixed dielectric were an uninsulated metallic ball (1219. &c.) or other properly shaped mass. Such curved lines are the consequences of the two electric forces arranged as I have assumed them to be: and, that the inductive force can be directed in such curved lines is the strongest proof of the presence of the two powers and the polar condition of the dielectric particles.

1680. I think it is evident that in the case stated, action at a distance can only result through an action of the contiguous conducting particles. There is no reason why the inductric body should polarize or affect *distant* conductors and leave those *near* it, namely the particles of the dielectric, unaffected: and everything in the form of fact and experiment with conducting masses or particles of a sensible size contradicts such a supposition.

1681. A striking character of the electric power is that it is limited and exclusive, and that the two forces being always present are exactly equal in amount. The forces are related in one of two ways, either as in the natural normal condition of an uncharged insulated conductor; or as in the charged state, the latter being a case of induction.

1682. Cases of induction are easily arranged so that the two forces being limited in their direction shall present no phenomena or indications external to the apparatus employed. Thus if a Leyden jar, having its external coating a little higher than the internal, be charged and then its charging ball and rod removed, such jar will present no electrical appearances so long as its outside is uninsulated. The two forces which may be said to be in the coatings, or in the particles of the dielectric contiguous to them, are entirely engaged to each other by induction through the glass; and a carrier ball (1181.) applied either to the inside or outside of the jar will show no signs of electricity. But if the jar be insulated, and the charging ball and rod, in an uncharged state and suspended by an insulating thread of white silk, be restored to their place, then the part projecting above the jar will give electrical indications and charge the carrier, and at the same time the *outside* coating of the jar will be found in the opposite state and inductric towards external surrounding objects.

1683. These are simple consequences of the theory. Whilst the charge of the inner coating could induce only through the glass towards the outer coating, and the latter contained no more of the contrary force than was equivalent to it, no induction external to the jar could be perceived; but when the inner coating was extended by the rod and ball so that it could induce through the air towards external objects, then the tension of the polarized glass molecules would, by their tendency to return to the normal state, fall a little, and a portion of the charge passing to the surface of this new part of the inner conductor, would produce inductive action through the air towards distant objects, whilst at the same time a part of the force in the outer coating previously directed inwards would now be at liberty, and indeed be constrained to

induct outwards through the air, producing in that outer coating what is sometimes called, though I think very improperly, free charge. If a small Leyden jar be converted into that form of apparatus usually known by the name of the electric well, it will illustrate this action very completely.

1684. The terms *free charge* and *dissimulated electricity* convey therefore erroneous notions if they are meant to imply any difference as to the mode or kind of action. The charge upon an insulated conductor in the middle of a room is in the same relation to the walls of that room as the charge upon the inner coating of a Leyden jar is to the outer coating of the same jar. The one is not more *free* or more *dissimulated* than the other, and when sometimes we make electricity appear where it was not evident before, as upon the outside of a charged jar when, after insulating it, we touch the inner coating, it is only because we divert more or less of the inductive force from one direction into another; for not the slightest change is in such circumstances impressed upon the character or action of the force.

1685. Having given this general theoretical view, I will now notice particular points relating to the nature of the assumed electric polarity of the insulating dielectric particles.

1686. The polar state may be considered in common induction as a forced state, the particles tending to return to their normal condition. It may probably be raised to a very high degree by approximation of the inductric and inducteous bodies or by other circumstances; and the phenomena of electrolyzation (861. 1652. 1706) seem to imply that the proportion of power which can thus be accumulated on a single particle is enormous. Hereafter we may be able to compare corpuscular forces, as those of gravity, cohesion, electricity, and chemical affinity, and in some way or other from their effects deduce their relative equivalents; at present we are not able to do so, but there seems no reason to doubt that their electrical, which are at the same time their chemical, forces (891. 918.) will be by far the most energetic.

1687. I do not consider the powers when developed by the polarization as limited to two distinct points or spots on the surface of each particle to be considered as the poles of an axis, but as resident on large portions of that surface, as they are upon the surface of a conductor of sensible size when it is thrown into a polar state. But it is very probable, notwithstanding, that the particles of different bodies may present specific differences in this respect, the powers not being equally diffused though equal in quantity; other circumstances also, as form and quality, giving to each a peculiar polar relation. It is perhaps to the existence of some such differences as these that we may attribute the specific actions of the different dielectrics in relation to discharge (1394. 1508.). Thus with respect to oxygen and nitrogen singular contrasts were presented when spark and brush discharge were made to take place in these gases, as may be seen by reference to the Table in paragraph 1518 of the Thirteenth Series; for

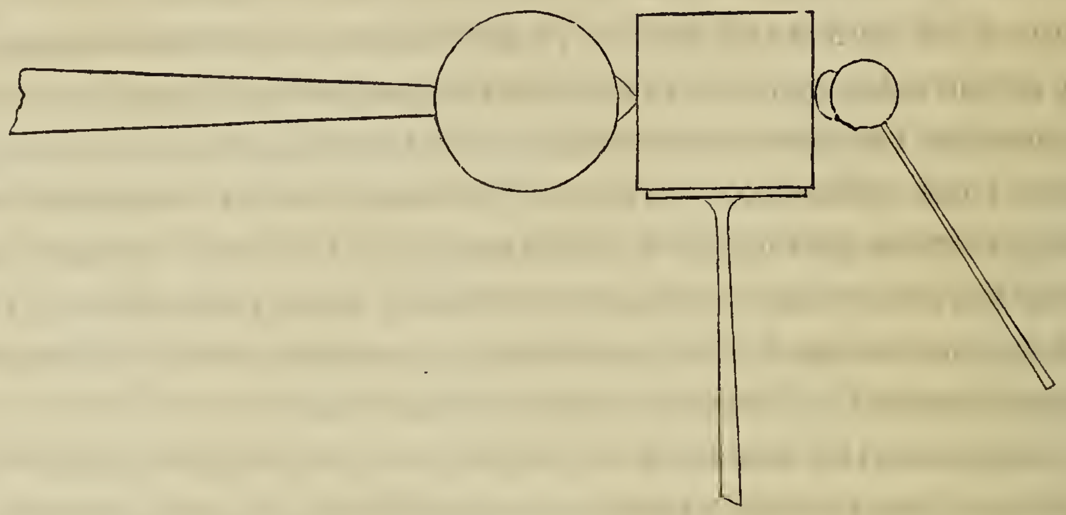
with nitrogen, when the small negative or the large positive ball was rendered inductric, the effects corresponded with those which in oxygen were produced when the small positive or the large negative was rendered inductric.

1688. In such solid bodies as glass, lac, sulphur, &c., the particles appear to be able to polarize in all directions, for a mass when experimented upon so as to ascertain its inductive capacity in three or more directions (1690.), gives no indication of a difference. Now as the particles are fixed in the mass, and as the direction of the induction through them must change with its change relative to the mass, the constant effect indicates that they can polarize electrically in any direction. This accords with the view already taken of each particle as a whole being a conductor (1669.), and, as an experimental fact, helps to confirm that view.

1689. But though particles may thus polarize in *any* direction under the influence of powers which are probably of extreme energy (1686.), it does not follow that each particle may not tend to polarize to a greater degree, or with more facility, in one direction than another; or that different kinds may not have specific differences in this respect, as they have differences of conducting and other powers (1296. 1326. 1395.). I sought with great anxiety for a relation of this nature; and selecting crystalline bodies as those in which all the particles are symmetrically placed, and therefore best fitted to indicate any result which might depend upon variation of the direction of the forces to the direction of the particles in which they were developed, experimented very carefully with them. I was the more strongly stimulated to this inquiry by the beautiful electrical condition of those crystalline bodies tourmaline and boracite, and hoped also to discover a relation between electric polarity and that of crystallization, or even of cohesion itself (1316.). My experiments have not established any connexion of the kind sought for. But as I think it of equal importance to show either that there is or is not such a relation, I shall briefly describe the results.

1690. The form of experiment was as follows. A brass ball 0·73 of an inch in diameter, fixed at the end of a horizontal brass rod, and that at the end of a brass cylinder, was by means of the latter connected with a large Leyden battery (291.) by perfect metallic communications, the object being to keep that ball, by its connexion with the charged battery in an electrified state, very nearly uniform, for half an hour at a time. This was the inductric ball. The inducteous ball was the carrier of the torsion electrometer (1229. 1314.); and the dielectric between them was a cube cut from a crystal, so that two of its faces should be parallel to the optical axis, whilst the other four were perpendicular to it. A small projecting piece of shell-lac was fixed on the inductric ball at that part opposite to the attachment of the brass rod, for the purpose of preventing actual contact between the ball and the crystal cube. A coat of shell-lac was also attached to that side of the carrier ball which was to be towards the cube, being also that side which was furthest from the repelled ball in the electrometer when placed in its position in that instrument. The cube was covered

with a thin coat of shell-lac dissolved in alcohol, to prevent the deposition of damp upon its surface from the air. It was supported upon a small table of shell-lac fixed



on the top of a stem of the same substance, the latter being of sufficient strength to sustain the cube, and yet flexible enough from its length to act as a spring, and allow the cube to bear, when in its place, against the shell-lac on the inductric ball.

1691. Thus it was easy to bring the inducteous ball always to the same distance from the inductric ball, and to uninsulate and insulate it again in its place; and then, after measuring the force in the electrometer (1181.), to return it to its place opposite to the inductric ball for a second observation. Or it was easy by revolving the stand which supported the cube to bring four of its faces in succession towards the inductric ball, and so observe the force when the lines of inductive action (1304.) coincided with, or were transverse to, the direction of the optical axis of the crystal. Generally from twenty to twenty-eight observations were made in succession upon the four vertical faces of a cube, and then an average expression of the inductive force was obtained, and compared with similar averages obtained at other times, every precaution being taken to secure accurate results.

1692. The first cube used was of *rock crystal*; it was 0·7 of an inch in the side. It presented a remarkable and constant difference, the average of not less than 197 observations giving 100 for the specific inductive capacity in the direction coinciding with the optical axis of the cube, whilst 93·59 and 93·31 were the expressions for the two transverse directions.

1693. But with a second cube of rock crystal corresponding results were not obtained. It was 0·77 of an inch in the side. The average of many experiments gave 100 for the specific inductive capacity coinciding with the direction of the optical axis, and 98·6 and 99·92 for the two other directions.

1694. Lord Ashley, whom I have found ever ready to advance the cause of science, obtained for me the loan of three globes of rock crystal belonging to Her Grace the Duchess of Sutherland for the purposes of this investigation. Two had such fissures as to render them unfit for the experiments (1193. 1698.). The third, which was very

superior, gave me no indications of any difference in the inductive force for different directions.

1695. I then used cubes of Iceland spar. One 0·5 of an inch in diameter gave 100 for the axial direction, and 98·66 and 95·74 for the two cross directions. The other, 0·8 of an inch in the side, gave 100 for the axial direction, whilst 101·73 and 101·86 were the numbers for the cross direction.

1696. Besides these differences there were others, which I do not think it needful to state, since the main point is not confirmed. For though the experiments with the first cube raised great expectation, they have not been generalized by those which followed. I have no doubt of the results as to that cube, but they cannot as yet be referred to crystallization. There are in the cube some faintly coloured layers parallel to the optical axis, and the matter which colours them may have an influence; but then the layers are also nearly parallel to a cross direction, and if at all influential should show some effect in that direction, which they did not.

1697. In some of the experiments one half or one part of a cube showed a superiority to another part, and this I could not trace to any charge the different parts had received. It was found indeed that the varnishing of the cubes was sufficient to prevent them receiving any charge, except (in a few experiments) a small degree of the negative state, or that which was contrary to the state of the inductric ball (1564. 1566.).

1698. I think it right to say that, as far as I could perceive, the insulating character of the cubes used was perfect, or at least so nearly perfect as to bear a comparison with shell-lac, glass, &c. As to the cause of the differences, other than regular crystalline structure, there may be several. Thus minute fissures in the crystal insensible to the eye may be so disposed as to produce a sensible electrical difference (1193.). Or the crystallization may be irregular: or the substance may not be quite pure; and if we consider how minute a quantity of matter will alter greatly the conducting power of water, it will seem not unlikely that a little extraneous matter diffused through the whole or part of a cube, may produce effects sufficient to account for all the irregularities of action that have been observed.

1699. An important inquiry regarding the electrical polarity of the particles of an insulating dielectric, is, whether it be the molecules of the particular substance acted on, or the component or ultimate particles, which thus act the part of insulated conducting polarizing portions (1669.).

1700. The conclusion I have arrived at is, that it is the molecules of the substance which polarize as wholes (1347.): and that however complicated the composition of a body may be, all those particles or atoms which are held together by chemical affinity to form one molecule of the resulting body, act as one conducting mass or particle when inductive phenomena and polarization are produced in the substance of which it is a part.

1701. This conclusion is founded on several considerations. Thus if we observe the insulating and conducting power of elements when they are used as dielectrics, we find some, as sulphur, phosphorus, chlorine, iodine, &c., whose particles insulate, and therefore polarize in a high degree; whereas others, as the metals, give scarcely any indication of possessing a sensible proportion of this power (1328.), their particles freely conducting one to another. Yet when these enter into combination they form substances having no direct relation apparently, in this respect, to their elements; for water, sulphuric acid, and such compounds formed of insulating elements, conduct by comparison freely; whilst oxide of lead, flint glass, borate of lead, and other metallic compounds containing very high proportions of conducting matter, insulate excellently well. Taking oxide of lead therefore as the illustration, I conceive that it is not the particles of oxygen and lead which polarize separately under the act of induction, but the molecules of oxide of lead which exhibit this effect, all the elements of one particle of the resulting body, being held together as parts of one conducting individual by the bonds of chemical affinity, which is but another term for electrical force (918.).

1702. In bodies which are electrolytes we have still further reason for believing in such a state of things. Thus when water, chloride of tin, iodide of lead, &c. in the solid state are between the electrodes of the voltaic battery, their particles polarize as those of any other insulating dielectric do (1164.); but when the liquid state is conferred on these substances the polarized particles divide, the two halves, each in a highly charged state, travelling onwards until they meet other particles in an opposite and equally charged state, with which they combine, to the neutralization of their chemical, i. e. their electrical forces, and the reproduction of compound particles, which can again polarize as wholes, and again divide to repeat the same series of actions (1347.).

1703. But though electrolytic particles polarize as wholes, it would appear very evident that in them it is not a matter of entire indifference *how* the particle polarizes (1689.), since, when free to move (380, &c.) the polarities are ultimately distributed in reference to the elements; and sums of force equivalent to the polarities, and very definite in kind and amount, separate, as it were, from each other, and travel onwards with the elementary particles. And though I do not pretend to know what an atom is, or how it is associated or endowed with electrical force, or how this force is arranged in the cases of combination and decomposition, yet the strong belief I have in the electrical polarity of particles when under inductive action, and the bearing of such an opinion on the general effects of induction, whether ordinary or electrolytic, will be my excuse, I trust, for a few hypothetical considerations.

1704. In electrolyzation it appears that the polarized particles would (because of the gradual change which has been induced upon the chemical, i. e. the electrical forces of their elements (918.)) rather divide than discharge to each other without division (1348.); for if their division, i. e. their decomposition and recombination, be

prevented by giving them the solid state, then they will insulate electricity perhaps a hundredfold more intense than that necessary for their electrolyzation (419, &c.). Hence the tension necessary for direct conduction in such bodies appears to be much higher than that for decomposition (419. 1164. 1344.).

1705. The remarkable stoppage of electrolytic conduction by solidification (380. 1358.), is quite consistent with these views of the dependence of that process on the polarity which is common to all insulating matter when under induction, though attended by such peculiar electro-chemical results in the case of electrolytes. Thus it may be expected that the first effect of induction is so to polarize and arrange the particles of water that the positive or hydrogen pole of each shall be from the positive electrode and towards the negative electrode, whilst the negative or oxygen pole of each shall be in the contrary direction; and thus when the oxygen and hydrogen of a particle of water have separated, passing to and combining with other hydrogen and oxygen particles, unless these new particles of water could turn round they could not take up that position necessary for their successful electrolytic polarization. Now solidification, by fixing the water particles and preventing them from assuming that essential preliminary position, prevents also their electrolysis; and so the transfer of forces in that manner being prevented (1347. 1703.), the substance acts as an ordinary insulating dielectric (for it is evident by former experiments (419. 1704.) that the insulating tension is higher than the electrolytic tension), induction through it rises to a higher degree, and the polar condition of the molecules as wholes, though greatly exalted, is still securely maintained.

1706. When decomposition happens in a fluid electrolyte, I do not suppose that all the molecules in the same sectional plane (1634.) part with and transfer their electrified particles or elements at once. Probably the *discharge force* for that plane is summed up on one or a few particles, which decomposing, travelling and recombining, restore the balance of forces, much as in the case of spark disruptive discharge (1406.); for as those molecules resulting from particles which have just transferred power must by their position (1705.) be less favourably circumstanced than others, so there must be some which are most favourably disposed, and these, by giving way first, will for the time lower the tension and produce discharge.

1707. In former investigations of the action of electricity (821, &c.) it was shown, for many satisfactory cases, that the quantity of electric power transferred onwards was in proportion to and was definite for a given quantity of matter moving as anion or cathion onwards in the electrolytic line of action; and there was strong reason to believe that each of the particles of matter then dealt with, had associated with it a definite amount of electrical force, constituting its force of chemical affinity, the chemical equivalents and the electro-chemical equivalents being the same (836.). It was also found with few, and I may now perhaps say with no exceptions (1341.), that only those compounds containing elements in single proportions could exhibit the characters and phenomena of electrolytes (697.); oxides, chlorides, and other bodies

containing more than one proportion of the electro-negative element refusing to decompose under the influence of the electric current.

1708. Probable reasons for these conditions and limitations arise out of the molecular theory of induction. Thus when a liquid dielectric, as chloride of tin, consists of molecules, each composed of a single particle of each of the elements, then as these can convey equivalent opposite forces by their separation in opposite directions, both decomposition and transfer can result. But when the molecules, as in the bichloride of tin, consist of one particle or atom of one element, and two of the other, then the simplicity with which the particles may be supposed to be arranged and to act, is destroyed. And, though it may be conceived that when the molecules of bichloride of tin are polarized as wholes by the induction across them, the positive polar force might accumulate on the one particle of tin whilst the negative polar force accumulated on the two particles of chlorine associated with it, and that these might respectively travel right and left to unite with other two of chlorine and one of tin, in analogy with what happens in cases of compounds consisting of single proportions, yet this is not altogether so evident or probable. For when a particle of tin combines with two of chlorine, it is difficult to conceive that there should not be some relation of the three in the resulting molecule analogous to fixed position, the one particle of metal being perhaps symmetrically placed in relation to the two of chlorine: and, it is not difficult to conceive of such particles that they could not assume that position dependent both on their polarity and the relation of their elements, which appears to be the first step in the process of electrolyzation (1345. 1705.).

§. 21. *Relation of the electric and magnetic forces.*

1709. I have already ventured a few speculations respecting the probable relation of magnetism, as the transverse force of the current, to the divergent or transverse force of the lines of inductive action belonging to static electricity (1658, &c.).

1710. In the further consideration of this subject it appeared to me to be of the utmost importance to ascertain, if possible, whether this lateral action which we call magnetism, or sometimes the induction of electrical currents (26. 1048, &c.), is extended to a distance *by the action of the intermediate particles* in analogy with the induction of static electricity, or the various effects, such as conduction, discharge, &c., which are dependent on that induction; or, whether its influence at a distance is altogether independent of such intermediate particles (1662.).

1711. I arranged two magneto-electric helices with iron cores end to end, but with an interval of an inch and three quarters between them, in which interval was placed the end or pole of a bar magnet. It is evident that on moving the magnetic pole from one core towards the other, a current would tend to form in both helices, in the one because of the lowering, and in the other because of the strengthening of the magnetism induced in the respective soft iron cores. The helices were connected together, and also with a galvanometer, so that these two currents should coincide in

direction, and tend by their joint force to deflect the needle of the instrument. The whole arrangement was so effective and delicate, that moving the magnetic pole about the eighth of an inch to and fro two or three times, in periods equal to those required for the vibrations of the galvanometer needle, was sufficient to cause considerable vibration in the latter; thus showing readily the consequence of strengthening the influence of the magnet on the one core and helix, and diminishing it on the other.

1712. Then without disturbing the distances of the magnet and cores, plates of substances were interposed. Thus calling the two cores A and B, a plate of shell-lac was introduced between the magnetic pole and A for the time occupied by the needle in swinging one way; then it was withdrawn for the time occupied in the return swing; introduced again for another equal portion of time; withdrawn for another portion, and so on eight or nine times; but not the least effect was observed on the needle. In other cases the plate was alternated, i. e. it was introduced between the magnet and A for one period of time, withdrawn and introduced between the magnet and B for the second period, withdrawn and restored to its first place for the third period, and so on, but with no effect on the needle.

1713. In these experiments *shell-lac* in plates 0·9 of an inch in thickness, *sulphur* in a plate 0·9 of an inch in thickness, and *copper* in a plate 0·7 of an inch in thickness were used without any effect. And I conclude that bodies, contrasted by the extremes of conducting and insulating power, and opposed to each other as strongly as metals, air, and sulphur, show no difference with respect to magnetic forces when placed in their lines of action, at least under the circumstances described.

1714. With a plate of iron, or even a small piece of that metal, as the head of a nail, a very different effect was produced, for then the galvanometer immediately showed its sensibility, and the perfection of the general arrangement.

1715. I arranged matters so that a plate of *copper* 0·2 of an inch in thickness, and ten inches in diameter, should have the part near the edge interposed between the magnet and the core, in which situation it was first rotated rapidly, and then held quiescent alternately, for periods according with that required for the swinging of the needle; but not the least effect upon the galvanometer was produced.

1716. A plate of shell-lac 0·6 of an inch in thickness was applied in the same manner, but whether rotating or not it produced no effect.

1717. Occasionally the plane of rotation was directly across the magnetic curve: at other times it was made as oblique as possible; the direction of the rotation being also changed in different experiments, but not the least effect was produced.

1718. I now removed the helices with their soft iron cores, and replaced them by two *flat helices* wound upon card board, each containing forty-two feet of silked copper wire, and having no associated iron. Otherwise the arrangement was as before, and exceedingly sensible; for a very slight motion of the magnet between the helices produced an abundant vibration of the galvanometer needle.

1719. The introduction of plates of shell-lac, sulphur, or copper into the intervals

between the magnet and these helices (1713.), produced not the least effect, whether the former were quiescent or in rapid revolution (1715.). So here no evidence of the influence of the intermediate particles could be obtained (1710.).

1720. The magnet was then removed and replaced by a flat helix, corresponding to the two former, the three being parallel to each other. The middle helix was so arranged that a voltaic current could be sent through it at pleasure. The former galvanometer was removed, and one with a double coil employed, one of the lateral helices being connected with one coil, and the other helix with the other coil, in such manner that when a voltaic current was sent through the middle helix its inductive action (26.) on the lateral helices should cause currents in them, having contrary directions in the coils of the galvanometer. By a little adjustment of the distances these induced currents were rendered exactly equal, and the galvanometer needle remained stationary notwithstanding their frequent production in the instrument. I will call the middle coil C, and the external coils A and B.

1721. A plate of copper 0·7 of an inch thick and six inches square, was placed between coils C and B, their respective distances remaining unchanged; and then a voltaic current from twenty pairs of 4-inch plates was sent through the coil C, and intermitted, in periods fitted to produce an effect on the galvanometer (1712.), if any difference had been produced in the effect of C on A and B. But notwithstanding the presence of air in one interval and copper in the other, the inductive effect was exactly alike on the two coils, and as if air had occupied both intervals. So that notwithstanding the facility with which any induced currents might form in the thick copper plate, the coil outside of it was just as much affected by the central helix C as if no such conductor as the copper had been there.

1722. Then, for the copper plate was substituted one of sulphur 0·9 of an inch thick; still the results were exactly the same, i. e. there was no action at the galvanometer.

1723. Thus it appears that when a voltaic current in one wire is exerting its inductive action to produce a contrary or a similar current in a neighbouring wire, according as the primary current is commencing or ceasing, it makes not the least difference whether the intervening space is occupied by such insulating bodies as air, sulphur and shell-lac, or such conducting bodies as copper, and the other non-magnetic metals.

1724. A correspondent effect was obtained with the like forces when resident in a magnet thus. A single flat helix (1718.) was connected with a galvanometer, and a magnetic pole placed near to it; then by moving the magnet to and from the helix, or the helix to and from the magnet, currents were produced indicated by the galvanometer.

1725. The thick copper plate (1721.) was afterwards interposed between the magnetic pole and the helix; nevertheless on moving these to and fro, effects, exactly the same in direction and amount, were obtained as if the copper had not been there. So also on introducing a plate of sulphur into the interval, not the least influence on the currents produced by motion of the magnet or coils could be obtained.

1726. These results, with many others which I have not thought it needful to de-

scribe, would lead to the conclusion that (judging by the amount of effect produced at a distance by forces transverse to the electric current, i. e. magnetic forces,) the intervening matter, and therefore the intervening particles, have nothing to do with the phenomena; or in other words, that though the inductive force of static electricity is transmitted to a distance by the action of the intermediate particles (1164. 1666.), the transverse inductive force of currents, which can also act at a distance, is not transmitted by the intermediate particles in a similar way.

1727. It is however very evident that such a conclusion cannot be considered as proved. Thus when the metal copper is between the pole and the helix (1715. 1719. 1725.) or between the two helices (1721.) we know that its particles are affected, and can by proper arrangements make their peculiar state for the time very evident by the production of either electrical or magnetical effects. It seems impossible to consider this effect on the particles of the intervening matter as independent of that produced by the inductric coil or magnet C, on the inducteous coil or core A (1715. 1721.); for since the inducteous body is equally affected by the inductric body whether these intervening and affected particles of copper are present or not (1723. 1725.), such a supposition would imply that the particles so affected had no reaction back on the original inductric forces. The more reasonable conclusion, as it appears to me, is, to consider these affected particles as efficient in continuing the action onwards from the inductric to the inducteous body, and by this very communication producing the effect of no loss of induced power at the latter.

1728. But then it may be asked what is the relation of the particles of insulating bodies, such as air, sulphur, or lac, when *they* intervene in the line of magnetic action? The answer to this is at present merely conjectural. I have long thought there must be a particular condition of such bodies corresponding to the state which causes currents in metals and other conductors (26. 53. 191. 201. 213.); and considering that the bodies are insulators one would expect that state to be one of tension. I have by rotating non-conducting bodies near magnetic poles and poles near them, and also by causing powerful electric currents to be suddenly formed and to cease around and about insulators in various directions, endeavoured to make some such state sensible, but have not succeeded. Nevertheless, as any such state must be of exceedingly low intensity, because of the feeble intensity of the currents which are used to induce it, it may well be that the state may exist, and may be discoverable by some more expert experimentalist, though I have not been able to make it sensible.

1729. It appears to me possible, therefore, and even probable, that magnetic action may be communicated to a distance by the action of the intervening particles, in a manner having a relation to the way in which the inductive forces of static electricity are transferred to a distance (1677.); the intervening particles assuming for the time more or less of a peculiar condition, which (though with a very imperfect idea) I have several times expressed by the term *electro-tonic state* (60. 242. 1114. 1661.). I hope it will not be understood that I hold the settled opinion that such is the case. I would

rather in fact have proved the contrary, namely, that magnetic forces are quite independent of the matter intervening between the inductric and the inducteous bodies; but I cannot get over the difficulty presented by such substances as copper, silver, lead, gold, carbon, and even aqueous solutions (201. 213.), which though they are known to assume a peculiar state whilst intervening between the bodies acting and acted upon (1727.), no more interfere with the final result than those which have as yet had no peculiarity of condition discovered in them.

1730. A remark important to the whole of this investigation ought to be made here. Although I think the galvanometer used as I have described it (1711. 1720.) is quite sufficient to prove that the final amount of action on each of the two coils or the two cores A and B (1713. 1719.) is equal, yet there is an effect which *may* be consequent on the difference of action of two interposed bodies which it would not show. As time enters as an element into these actions* (125.), it is very possible that the induced actions on the helices or cores A, B, though they rise to the same degree when air and copper, or air and lac are contrasted as intervening substances, do not do so in the same time; and yet, because of the length of time occupied by a vibration of the needle, this difference may not be visible, both effects rising to their maximum in periods so short as to make no sensible portion of that required for a vibration of the needle, and so exert no visible influence upon it.

1731. If the lateral or transverse force of electrical currents, or what appears to be the same thing, magnetic power, could be proved to be influential at a distance independently of the intervening contiguous particles, then, as it appears to me, a real distinction, of a high and important kind, would be established between the natures of these two forces (1654. 1664.). I do not mean that the powers are independent of each other and might be rendered separately active, on the contrary they are probably essentially associated (1654.), but it by no means follows that they are of the same nature. In common statical induction, in conduction, and in electrolyzation, the forces at the opposite extremities of the particles which coincide with the lines of action, and have commonly been distinguished by the term electric, are polar, and in the cases of contiguous particles act only to insensible distances; whilst those which are transverse to the direction of these lines, and are called magnetic, are circumferential, act at a distance, and if not through the intermediation of the intervening particles, have their relations to ordinary matter entirely unlike those of the electrical forces with which they are associated.

1732. To decide this question of the identity or distinction of the two kinds of power, and establish their true relation, would be exceedingly important. The question seems fully within the reach of experiment, and offers a high reward to him who will attempt its settlement.

* See Annales de Chimie, 1833, tom. li. pp. 422, 428.

1733. I have already expressed a hope of finding an effect or condition which shall be to statical electricity what magnetic force is to current electricity (1658.) If I could have proved to my own satisfaction that magnetic forces extended their influence to a distance by the conjoined action of the intervening particles in a manner analogous to that of electrical forces, then I should have thought that the lateral tension of the lines of inductive action (1659.), or that state so often hinted at as the electro-tonic state (1661. 1662.), was this related condition of statical electricity.

1734. It may be said that the state of *no lateral action* is to static or inductive force the equivalent of *magnetism* to current force; but that can only be upon the view that electric and magnetic action are in their nature essentially different (1664.). If they are the same power, the whole difference in the results being the consequence of the difference of *direction*, then the normal or *undeveloped* state of electric force will correspond with the state of *no lateral action* of the magnetic state of the force; the electric current will correspond with the lateral effects commonly called magnetism: but the state of static induction which is between the normal condition and the current will still require a corresponding lateral condition in the magnetic series, presenting its own peculiar phenomena; for it can hardly be supposed that the normal electric and the inductive or polarized electric condition can both have the same lateral relation. If magnetism be a separate and a higher relation of the powers developed, then perhaps the argument which presses for this third condition of that force would not be so strong.

1735. I cannot conclude these general remarks upon the relation of the electric and magnetic forces without expressing my surprise at the results obtained with the copper plate (1721. 1725.). The experiments with the flat helices represent one of the simplest cases of the induction of electrical currents (1720.); the effect, as is well known, consisting in the production of a momentary current in a wire at the instant when a current in the contrary direction begins to pass through a neighbouring parallel wire, and the production of an equally brief current in the reverse direction when the determining current is stopped (26.). Such being the case, it seems very extraordinary that this induced current which takes place in the helix A when there is only air between A and C (1720.) should be equally strong when that air is replaced by an enormous mass of that excellently conducting metal copper (1721.). It might have been supposed that this mass would have allowed of the formation and discharge of almost any quantity of currents in it, which the helix C was competent to induce, and so in some degree have diminished if not altogether prevented the effect in A: instead of which, though we can hardly doubt that an infinity of currents are formed at the moment in the copper plate, still not the smallest diminution or alteration of the effect in A appears. Almost the only way of reconciling this effect with generally received notions is, as it appears to me, to admit that magnetic action is communicated by the action of the intervening particles (1729. 1733.).

1736. This condition of things, which is very remarkable, accords perfectly with

the effects observed in solid helices where wires are coiled over wires to the amount of five or six or more layers in succession, no diminution of effect on the outer ones being occasioned by those within.

§. 22. *Note on electrical excitation.*

1737. That the different modes in which electrical excitement takes place will some day or other be reduced under one common law can hardly be doubted, though for the present we are bound to admit distinctions. It will be a great point gained when these distinctions are, not removed but, understood.

1738. The strict relation of the electrical and chemical powers renders the chemical mode of excitement the most instructive of all, and the case of two isolated combining particles is probably the simplest that we possess. Here however the action is local, and we still want such a test of electricity as shall apply to it, to cases of current electricity, and also to those of static induction. Whenever by virtue of the previously combined condition of some of the acting particles (923.) we are enabled, as in the voltaic pile, to expand or convert the local action into a current, then chemical action can be traced through its variations to the production of *all* the phenomena of tension and the static state, these being in every respect the same as if the electric forces producing them had been developed by friction.

1739. It was Berzelius, I believe, who first spoke of the aptness of certain particles to assume opposite states when in presence of each other (959.). Hypothetically we may suppose these states to increase in intensity by increased approximation, or by heat, &c. until at a certain point combination occurs, accompanied by such an arrangement of the forces of the two particles between themselves as is equivalent to a discharge, producing at the same time a particle which is throughout a conductor (1700.).

1740. This aptness to assume an excited electrical state (which is probably polar in those forming non-conducting matter) appears to be a primary fact, and to partake of the nature of induction (1162.), for the particles do not seem capable of retaining their particular state independently of each other (1177.) or of matter in the opposite state. What appears to be definite about the particles of matter is their assumption of a *particular* state, as the positive or negative, in relation to each other, and not of either one or other indifferently; and also the acquirement of force up to a certain amount.

1741. It is easily conceivable that the same force which causes local action between two free particles shall produce current force if one of the particles is previously in combination, forming part of an electrolyte (923. 1738.). Thus a particle of zinc, and one of oxygen, when in presence of each other, exert their inductive forces (1740.), and these at last rise up to the point of combination. If the oxygen be previously in union with hydrogen, it is held so combined by an analogous exertion and arrangement of the forces; and as the forces of the oxygen and hydrogen are for the time of combination mutually engaged and related, so when the superior relation of the forces be-

tween the oxygen and zinc come into play, the induction of the former or oxygen towards the metal cannot be brought on and increased without a corresponding deficiency in its induction towards the hydrogen with which it is in combination (for the amount of force in a particle is considered as definite), and the latter therefore has its force turned towards the oxygen of the next particle of water; thus the effect may be considered as extended to sensible distances, and thrown into the condition of static induction, which being discharged and then removed by the action of other particles produces currents.

1742. In the common voltaic battery, the current is occasioned by the tendency of the zinc to take the oxygen of the water from the hydrogen, the effective action being at the place where the oxygen leaves the previously existing electrolyte. But Schœnbein has arranged a battery in which the effective action is at the other extremity of this essential part of the arrangement, namely, where oxygen goes to the electrolyte. The first may be considered as a case where the current is put into motion by the abstraction of oxygen from hydrogen, the latter by that of hydrogen from oxygen. The direction of the electric current is in both cases the same, when referred to the direction in which the elementary particles of the electrolyte are moving (923. 962.), and both are equally in accordance with the hypothetical view of the inductive action of the particles just described (1740.).

1743. In such a view of voltaic excitement, the action of the particles may be divided into two parts, that which occurs whilst the force in a particle of oxygen is rising towards a particle of zinc acting on it, and falling towards the particle of hydrogen with which it is associated (this being the progressive period of the inductive action), and that which occurs when the change of association takes place, and the particle of oxygen leaves the hydrogen and combines with the zinc. The former appears to be that which produces the current, or if there be no current, produces the state of tension at the termination of the battery; whilst the latter, by terminating for the time the influence of the particles which have been active, allows of others coming into play, and so the effect of current is continued.

1744. It seems highly probable, that excitement by friction may very frequently be of the same character. Wollaston endeavoured to refer such excitement to chemical action*; but if by chemical action ultimate union of the acting particles is intended, then there are plenty of cases which are opposed to such a view. Davy mentions some such, and for my own part I feel no difficulty in admitting other means of electrical excitement than chemical action, especially if by chemical action is meant a final combination of the particles.

1745. Davy refers experimentally to the opposite states which two particles having opposite chemical relations can assume when they are brought into the close vicinity of each other, but *not* allowed to combine†. This, I think, is the first part of the action already described (1743.); but in my opinion it cannot give rise to a con-

* Philosophical Transactions, 1801, p. 427. † Ibid. 1807. p. 34.

tinuous current unless combination take place, so as to allow other particles to act successively in the same manner, and not even then unless one set of the particles be present as an element of an electrolyte (923. 963.); i. e. mere quiescent contact alone without chemical action does not in such cases produce a *current*.

1746. Still it seems very possible that such a relation may produce a high charge, and thus give rise to excitement by friction. When two bodies are rubbed together to produce electricity in the usual way, one at least must be an insulator. During the act of rubbing, the particles of opposite kinds must be brought more or less closely together, the few which are most favourably circumstanced being in such close contact as to be short only of that which is consequent upon chemical combination. At such moments they may acquire by their mutual induction (1740.) and partial discharge to each other very exalted opposite states, and when, the moment after, they are by the progress of the rub removed from each other's vicinity, they will retain this state if both bodies be insulators, and exhibit them upon their complete separation.

1747. All the circumstances attending friction seem to me to favour such a view. The irregularities of form and pressure will cause that the particles of the two rubbing surfaces will be at very variable distances, only a few at once being in that very close relation which is probably necessary for the development of the forces; further, those which are nearest at one time will be further removed at another, and others will become the nearest, and so by continuing the friction many will in succession be excited. Finally, the lateral direction of the separation in rubbing seems to me the best fitted to bring many pairs of particles, first of all into that close vicinity necessary for their assuming the opposite states by relation to each other, and then to remove them from each other's influence whilst they retain that state.

1748. It would be easy, on the same view, to explain hypothetically, how, if one of the rubbing bodies be a conductor, as the amalgam of an electrical machine, the state of the other when it comes from under the friction is (as a mass) exalted; but it would be folly to go far into such speculation before that already advanced has been backed or corrected by fit experimental evidence. I do not wish it to be supposed that I think all excitement by friction is of this kind; on the contrary, certain experiments lead me to believe that in many cases, and perhaps in all, effects of a thermo-electric nature conduce to the ultimate end; and there are very probably other causes of electric disturbance influential at the same time, which we have not as yet distinguished.

Royal Institution,
June, 1838.

XIV. *An Account of some Experiments on the Blood in connexion with the Theory of Respiration. By* John Davy, *M.D. F.R.S., Assistant Inspector of Army Hospitals.*

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Connected with the theory of respiration and of animal heat there are many questions of interest respecting the blood, about which physiologists differ in opinion, and which consequently are fit subjects for further inquiry.

Some of the more important and fundamental of these questions I have endeavoured to investigate experimentally, and in the present communication I propose to submit to the Royal Society the results which I have obtained, with the hope that they may be considered not unworthy of a place in the Philosophical Transactions.

I. *Is blood capable of absorbing oxygen independent of putrefaction?*

To endeavour to satisfy myself on this point, on which in a former inquiry I had arrived at a negative conclusion in opposition to the commonly received opinion, I have employed two methods of experimenting: one, of agitating blood, recently drawn and rapidly cooled in common air and in oxygen, in a tube of the capacity of two cubic inches, divided into a hundred parts; the other, of agitating it in larger quantities with the same airs, in the very convenient apparatus employed by Dr. Christison when engaged in a similar inquiry, consisting of a double tubulated bottle of the capacity of thirty-two cubic inches, provided with stop-cocks adapted by grinding, to one of which a moveable bent glass tube was fitted to connect it with a pneumatic trough, and to the other a perpendicular tube surmounted by a funnel.

The blood subjected to experiment in every instance was prepared by the displacement of its fibrin. This was done by agitating it with small pieces of sheet-lead in a bottle filled to overflowing and closed with a cork, enveloped in moist bladder and covered over with the same, tied round the neck of the bottle, so as to exclude atmospheric air, whilst in the act of coagulating and of cooling, and to allow, when cooled, of the withdrawal of the cork.

Prepared thus, and rapidly cooled, I have tried different specimens of blood, venous and arterial, of Man, of the Sheep, Ox, Dog and Cat; and the results, when the blood has been taken from a healthy animal, have been decisive and consistent. In every instance, whether atmospheric air or oxygen was used, after agitation, there was a marked diminution of the volume of air.

In the examples which it may be advisable to bring forward, I shall confine myself to a few of the experiments I have made on the blood of the Sheep.

Using the graduated tube over mercury, sixty-two measures of arterial blood from

the carotid artery, agitated with thirty-three measures of common air, produced a diminution of two measures; and sixty-three of the same arterial blood, agitated with thirteen of pure oxygen, a diminution of three; whilst sixty-three of venous blood from the jugular vein of the same animal, agitated with thirty-three of common air, produced a diminution of six; and seventy of this blood, with thirteen of oxygen, a diminution of eight*.

In experiments on a larger scale, using the double-mouthed bottle, in which about ten cubic inches of blood were agitated with about twenty-two cubic inches of air, the results were in accordance with the preceding. Thus when the arterial blood of the Sheep was agitated with common air and with oxygen, on turning the stop-cock of the bent tube there was an absorption in one instance of about ·3 cubic inch, and in the other of about ·4 cubic inch; and, with venous blood, in the instance of common air, of about 1· cubic inch, and in that of oxygen, of about 1·25 cubic inch.

These experiments were all made on the blood of the same animal. In experiments on the blood of different individuals of the same species of animal, and on the blood of animals of different species, the results have varied in regard to the degree of absorption, and remarkably so in the instance of the blood of man.

In every instance the absorption or disappearance of a portion of the air has been attended with some change in the colour of the blood; the venous has invariably acquired the florid vermillion hue of arterial blood, and the arterial has had its florid hue heightened.

The air that has been absorbed or which disappears, when atmospheric air has been used, in accordance with the commonly received opinion and the results of Dr. Christison's experiments†, has been found to be oxygen.

Relative to the residual air, when pure oxygen has been used, whether on the smaller or larger scale of experiment, no carbonic acid gas has been detected in it in the most carefully conducted trials. When common air has been employed, then a trace of this acid gas has been found in the residual air after agitation with the blood, but not exceeding 1 per cent. at a temperature between 40° and 50°. I shall notice in detail an experiment with each, as the results are of consequence.

Ten cubic inches of venous blood (its fibrin displaced) from the jugular vein of a Sheep, rapidly cooled, were agitated in twenty-two cubic inches of oxygen gas, which had been well washed with lime-water. After an absorption equal to about one cubic inch, some of the gas was expelled and passed into lime-water, the transparency of which it did not in the slightest degree impair.

The same quantity of similar blood was similarly treated with common air, which

* Notwithstanding the frothing attending the agitation of blood in air, the absorption or diminution of volume was ascertained with tolerable accuracy by observing the rise of the mercury in the tube. The experiments were made as nearly as possible under ordinary atmospheric pressure, which was easily effected, as the mercurial pneumatic trough used exceeded in depth the length of the tube.

† Edinburgh Medical and Surgical Journal, vol. xxxv. p. 94.

also had been washed with lime-water. After an absorption equal to about one cubic inch, some of the air was passed into lime-water: it occasioned a just perceptible cloudiness; and eighty measures of it agitated with lime-water were reduced hardly to 79·5.

Taking it for granted that this very minute quantity of carbonic acid derived from the blood existed in the state of gas, and not contained in the aqueous vapour, as is possible, it is matter for consideration from whence it was derived,—whether it was formed at the moment by the action of the air on the blood, or, previously existing in the blood, was now merely expelled. Further on I propose to return to this important question.

When I reflect on the results stated above relative to the absorption or disappearance of oxygen, and compare them with those alluded to formerly obtained, I am not a little surprised at their discrepancy; and I can only account for it by supposing that it may have been owing in part to the difference of season when the two sets of experiments were conducted. The first were made in Malta in 1829, in the hot months of July and August, when the thermometer in the open air was generally above 80° and occasionally above 90°. The last have been made in England, and principally in January of the present year, when the temperature of atmosphere was occasionally low, the greater part of the time below the freezing point, and often as low as 20°. From what I have witnessed, I am induced to infer, that the higher the atmospheric temperature is, and the less necessity there is for the production of animal heat, the less difference there is between venous and arterial blood, and the less power the former has of combining with oxygen and of forming or evolving carbonic acid. In Malta I carefully compared the blood of the jugular vein and carotid artery of a Sheep during the season mentioned; when coagulated and still hot, there was no perceptible difference in their colour; in each it was less florid than the arterial blood of the same animal in England in winter, and less dark than the venous: its hue was as it were a mixture of the two. And in this observation I could not be mistaken; the circumstances under which it was made precluded mistake; vessels of the same size were used; similar quantities of blood were introduced, and they were seen in the same light side by side. And in confirmation of this view I may remark, that during the last winter, when the cold was unusually severe, I found the temperature of deeply seated parts, and especially of the heart and its left ventricle, in the instance of Sheep, unusually high: the mean of nine observations on the temperature of the left ventricle in different animals was 107·5; the lowest was 105·5; the highest 109·; whilst the temperature of the rectum (the mean of the same number of observations) was 104·4.

II. *Does the blood, especially venous blood, contain carbonic acid capable of being expelled by agitation with another gas, as hydrogen or oxygen?*

In a paper published in the Philosophical Transactions for 1832, Dr. Stevens has answered this question in the affirmative: he maintains that carbonic acid gas exists

in venous blood; that it may be expelled by oxygen or by hydrogen, although not by the air-pump; and he supposes that the difference of effect is owing to a peculiar attraction for carbonic acid exercised by these gases.

To endeavour to resolve my doubts on this important point, I have had recourse to the apparatus already mentioned, viz. the graduated tube with the mercurial pneumatic trough, and the double-mouthed bottle furnished with stop-cocks, &c. as being well adapted for simple and decisive experiments.

By means of the graduated tube I have agitated venous blood in hydrogen over mercury, as about a cubic inch of each, and other proportions, and have left the blood exposed to the influence of the gas for several hours; and I have made similar trials with it, using larger quantities in the double-mouthed bottle, as sixteen cubic inches of each, and also other proportions; the results have been either of the same negative character, or, if different, indicating only the disengagement of carbonic acid gas in an extremely minute quantity. In all the experiments with the graduated tube in which fresh blood was used, whether of Man or of the Sheep, the fibrin displaced out of the contact of the air, on agitation with hydrogen, there was no sensible increase of the volume of the gas, and no diminution of it when it was transferred to, and shaken with, lime-water. And in the best experiments, on a larger scale, with the double-mouthed bottle, when most attention was paid to all the circumstances likely to insure accuracy, as in the first instance the exclusion of air from the blood, and in the second the having it of the temperature of the bottle and of the room, the results have been similar, and of a negative character. I shall describe a small number of experiments, those, the results of which appeared least ambiguous.

Seven cubic inches of Sheep's blood from the jugular vein, its fibrin broken up by agitation with lead whilst coagulating, out of contact with air, and cooled under water, were agitated with hydrogen (twenty-five cubic inches), previously well washed with lime-water, and which, tested by lime-water, after this precaution were found perfectly free from carbonic acid. On turning the stop-cock of the bent tube connected with distilled water, no change of volume was indicated, and the blood was agitated again with the same result. By means of the perpendicular tube distilled water was admitted, and some of the gas expelled; first a cubic inch into a graduated tube filled with lime-water, and next about four cubic inches into a vial filled with distilled water, and in which afterwards a little lime-water was added to the gas; in neither could any traces of carbonic acid be detected; the lime-water remained transparent.

Ten cubic inches of venous blood, taken by a large orifice from the arm of a young Man threatened with hæmoptysis, the fibrin broken up in the same manner as the last, and rapidly cooled under water in a running stream to the temperature of the room, 51°, were similarly treated with hydrogen, and with precisely the same result; after having been twice well shaken, on turning the stop-cock, there was no change of volume. The blood was kept in contact with the hydrogen over night, the stop-

cocks closed. The night, that of the 23rd of January, was severely cold; at 11 o'clock the following morning the temperature of the room was only 45°; now on turning the stop-cock of the bent tube the water rose in it to the extent of about one-eighth of a cubic inch.

Twenty-four cubic inches of the mixed arterial and venous blood of the Sheep, collected and prepared with similar precautions, were divided into two portions of about twelve cubic inches; one was agitated in the double-mouthed bottle with hydrogen after the introduction of a little milk of lime, the other without this addition. The result in each instance was the same; on opening the stop-cock, after the agitation, the water rose a very little in the bent tube, about one-twentieth of a cubic inch.

The results of some of the trials already described on the action of oxygen on venous blood, both pure and mixed with azote, in the form of common air, are very consistent with those just detailed on hydrogen. Previously to stating some other results in quest of fresh evidence on the same subject, it may be advisable to notice particularly the power which blood possesses of absorbing carbonic acid.

From experiments which I made on blood and serum in 1824 and 1828*, I inferred that each is capable of absorbing about an equal volume of this acid gas. I now find that when pure carbonic acid gas is brought in contact with blood or serum over mercury, and moderately agitated under ordinary atmospheric pressure, that the absorption of gas exceeds the volume of the fluid, both in the instance of blood and serum. The results of some experiments are exhibited in the following Table. The majority of them were obtained between a temperature of 40° and 45°; the three last at about 51°. When the venous and arterial blood, and the serum tried, were from the same animal, the numbers expressing the results are entered in the same line.

Volume of carbonic acid gas absorbed by 100 *parts of blood and serum.*

No.	Animal.	Venous blood.	Venous serum.	Arterial blood.	Arterial serum.
1	Sheep......	160	......	167	
2	Sheep......	155			
3	Sheep......	......	......	133	
4	Sheep......	142			
5	Sheep......	......	......	150	
6	Sheep......	166			
7	Sheep......	......	......	......	120
8	Ox........	194			
9	Ox........	181			
10	Man	118			
11	Heifer......	120	......	......	117
12	Sheep......	148	125	141	125
13	Sheep......	......	118	......	118
14	Man	153			
15	Sheep......	160			
16	Sheep......	......	......	159	

* Philosophical Transactions for 1824. Edinburgh Medical and Surgical Journal, vol. xxx.

The effect of the absorption of the gas to perfect saturation was on the arterial and venous blood the same; it rendered both very dark; the serum it rendered more liquid, which was well marked by diminished tendency to froth on agitation.

I shall now proceed to notice the trials which I have instituted of agitating blood and serum, to which a known quantity of carbonic acid had been added, with one or more of the gases considered by Dr. Stevens as exerting an attraction on carbonic acid, and by that means expelling it.

From the experiments which I have made on serum, it appears in its healthy state incapable of absorbing oxygen, or of immediately furnishing carbon to form carbonic acid; and in no instance in which I have agitated it with common air or with hydrogen, when obtained from the blood of a healthy animal, has there been any indication of the disengagement of gas; it therefore is peculiarly well fitted for the trial in question.

To nine cubic inches of serum from the mixed blood of the Sheep one cubic inch more of serum was added, containing a cubic inch of carbonic acid, with which it had been impregnated over mercury. The mixture of the two was introduced into the double-mouthed bottle without delay, and well agitated with twenty-two cubic inches of common air. On turning the stop-cock there was no change of volume. The serum was transferred, and there was added to it, with as little motion as possible, another cubic inch of serum, containing the same quantity of carbonic acid. Now poured back into the bottle and agitated, on opening the stop-cock a little air was disengaged; it was collected and found equal to $\frac{4}{100}$ths cubic inch. The serum was left exposed to the action of the air in the bottle over night, the stop-cocks closed; the following morning on opening the stop-cock of the bent tube no air was expelled; on the contrary, there was a just perceptible rise of water in it. The experiment was carried further: the serum was transferred to a vial and closed, and the double-mouthed bottle was filled with hydrogen. The serum was returned and well agitated with the hydrogen. On turning the stop-cock $\frac{114}{100}$ths of a cubic inch of air was expelled. It was agitated a second time without further expulsion of air, and left in contact with hydrogen for more than twelve hours without any further effect. Thus it appears that of the two cubic inches of carbonic acid gas introduced into the serum, only one-fifth of a cubic inch was expelled by successive agitation with atmospheric air and with hydrogen.

One cubic inch of venous blood of a Man which had absorbed 1·2 cubic inch of carbonic acid, was mixed with twelve cubic inches of similar blood, and agitated with hydrogen in the double-mouthed bottle. A very little air only was expelled, viz. $\frac{4}{100}$ths cubic inch.

To fifty-five measures (1·1 cubic inch) of venous blood of a Sheep, twenty measures of gas were added over mercury, composed of about equal parts of oxygen and carbonic acid. After agitation about seventeen measures were absorbed, and the blood had acquired a florid hue; ten measures more of oxygen were added; there was no

further absorption. The tube was transferred to lime-water and agitated; the residual air was not diminished; it amounted to thirteen or fourteen measures (the froth prevented precision in marking the quantity), which possessed the properties of oxygen, as tested by the taper.

To twenty-eight measures of venous blood of a Sheep, which had absorbed twenty-six of carbonic acid, forty-nine of oxygen were added in the graduated tube over mercury: after agitation the blood had acquired the florid arterial hue, and there appeared to be an expansion of one measure. Transferred to lime-water there was an absorption of one or two measures, and no more.

I shall notice one experiment more, in which blood nearly saturated with carbonic acid was exposed to oxygen, a membrane intervening. Forty-seven measures of venous blood, which had absorbed thirty-three of carbonic acid, were introduced into a glass tube half an inch in diameter, closed at one end with gold-beater's skin, and when filled with blood at the other end also, it was placed over mercury in a small receiver, and thirty-seven measures of oxygen were added to it, under a diminished pressure of about one inch. After twenty-four hours there was no change of volume; the blood in the tube had acquired throughout the arterial hue; the gas, thirty-eight measures, transferred to lime-water and agitated, diminished to thirty-two.

The tube was now placed under a receiver, and the air exhausted by the air-pump; a good deal of air was disengaged in the form of froth, and the gold-beater's skin was so distended that it appeared ready to burst; after three or four minutes air was re-admitted; a notable portion of gas was found free between the membrane and blood; thus showing that in oxygen gas carbonic acid gas is less freely exhaled through a membrane than in vacuo.

The results of this second set of experiments are in accordance with those of the first. The inference I am induced to draw from both is rather of a negative kind, and unfavourable to the conclusion of Dr. STEVENS already referred to, at least in a strict and general sense. I think it right to express myself thus reservedly, reflecting on some of the experiments in which a very little carbonic acid gas appeared to be extricated on agitating blood with hydrogen; and believing that Dr. STEVENS, and other able inquirers, could not have been misled on a point so little exposed to fallacy.

Relative to the effects which Dr. STEVENS refers to the attraction of one gas for another, they appear to me, from what I have witnessed in carrying on the inquiry, to admit of explanation on Dr. DALTON's theory of mixed gases, and that in no instance is the effect of disengagement of air from a fluid, agitated with another kind of air, greater than were it agitated in a vacuum.

III. *What is the condition of the alkali in the blood in relation to carbonic acid?*

On this point much difference of opinion exists amongst inquirers; some believing that the alkali, or at least a portion of it, is uncombined, or combined merely with water, or with water and albumen; some that it is united to carbonic acid in the

state of carbonate; others that it is saturated with this gas, and in the state of bicarbonate.

The subject, it must be confessed, is one of great difficulty, and very perplexing; partly from the nature of the blood, liable to great variations during life, and to rapid change after death; and partly also from the nature of the alkaline carbonates, hardly less disposed to change than the blood itself, from variation of circumstances, and to pass from one degree of combination into another.

The bicarbonate of soda, I believe, like the bicarbonate of ammonia, can only exist in perfection in the solid state. In dissolving, I find, when exposed to the atmosphere, it gives off a part of its acid, and still more when it is agitated with common air, and more still with hydrogen, and in a greater degree the higher the temperature. This is not favourable to the idea that it exists in the blood, especially when it is considered that this fluid may be exposed to a temperature of 212° without disengaging carbonic acid, of which I have had proof in several trials.

Sulphuretted hydrogen does not expel carbonic acid from the alkalies in solution in water. Bicarbonate of soda dissolved to saturation in distilled water absorbs, I find, 143 per cent. of its volume of this gas; whilst the serum of blood (it was Sheep's that was tried) dissolved 207 per cent., arterial blood 235, and venous 290. This, too, is unfavourable to the same idea; as is also the large proportion of carbonic acid which blood, it has been shown, is capable of absorbing.

Supertartrate of potash occasions an effervescence, when mixed in substance with a solution of the sesquicarbonate, but not of the carbonate of soda; and the effect is similar, whether the mixture be made over mercury, air excluded, or in an open vessel exposed to the atmosphere. The supertartrate of potash also, I find, mixed with blood and agitated with common air, acts as with the sesquicarbonate, and occasions a disengagement of air, and both from arterial and venous blood, and from serum; and the air I have ascertained is carbonic acid.

From these facts, may it not be inferred that the alkali in the blood, in its normal or healthiest condition, is neither in the state of carbonate nor of bicarbonate, but of sesquicarbonate? The power of the blood to absorb carbonic acid and sulphuretted hydrogen accords best with this view, and some other important properties of the fluid are, I believe, in harmony with it.

The sesquicarbonate, I may add, seems to be the state of rest of the alkali in combination with carbonic acid, under ordinary circumstances of exposure to the atmosphere. Thus the native compound is the sesquicarbonate, as is also, I believe, the effloresced salt*. And I find that although a solution of the bicarbonate may be brought by the air-pump to the state of sesquicarbonate, it cannot be reduced to that of the carbonate: after it has ceased to give off any air in vacuo, it effervesces

* On exposing carbonate of soda in excess to carbonic acid gas over mercury, the gas is rapidly absorbed with the expulsion of part of the water of crystallization, so as to produce an appearance of deliquescence, and the sesquicarbonate is formed.

with the supertartrate of potash; and if evaporated to dryness over sulphuric acid under an exhausted receiver, on being subjected to heat, it disengages carbonic acid gas.

IV. *Does the blood contain any gas capable of being extricated by the air-pump?*

On this subject also there has been much difference of opinion. Our distinguished countryman MAYOW, more than a century ago, stated that blood, especially arterial blood, effervesces in vacuo, which he attributed to the disengagement of air, his Spiritus Nitro-aëreus*. Sir EVERARD HOME, on the authority of Mr. BRANDE, in 1818, asserted that blood, both venous and arterial, under the exhausted receiver evolves a large quantity of carbonic acid gas, an ounce of blood as much as two cubic inches of gas†. I repeated the experiment shortly after, but without confirming the result; neither by the air-pump, nor by heat applied even to coagulation of blood and serum in close vessels, did I succeed in demonstrating the extrication of this acid‡. Since that time the experiment of the air-pump on the blood has been frequently made, and by observers of great accuracy, as by Drs. DUNCAN and CHRISTISON in this country, by MM. TIEDEMAN, GMELIN, MITSCHENLICH and MÜLLER on the continent, and recently by MM. BISCHOFF and MAGNUS. With the exception of the last-mentioned inquirers, the results have been negative. MM. BISCHOFF and MAGNUS, on the contrary, state that by careful exhaustion they have obtained gas from the blood; the former a small quantity of carbonic acid gas, the latter a notable quantity, and not only of carbonic acid gas, but also of oxygen and azote§.

M. MAGNUS attributes the failures of former experimenters to their having used pumps of imperfect exhausting power, or to their not having carried the exhaustion sufficiently far. In my early and first trial I employed the air pump belonging to the laboratory of the Royal Institution, which was an excellent one, and which I then believed was in good order; but I might have been mistaken.

To endeavour to satisfy myself on this point, I have had an air-pump constructed for the purpose, under the direction of an able artist, Mr. Ross, of 33 Regent Street, already distinguished for his excellent achromatic microscopes, and it has answered perfectly. When the exhaustion has been carried as far as possible, the difference of level of the mercury in the siphon-gauge has not exceeded a quarter of an inch, and over water has not exceeded half an inch.

Experimenting with this machine on blood, collected with such precautions as I believe to have been adequate to insure accuracy of results, in a majority of instances the disengagement of gas has been rendered manifest, and both from arterial and venous blood.

* JOHANNIS MAYOW Opera omnia. Hagæ Com. 1681. p. 133.

† Philosophical Transactions for 1818, p. 181.

‡ Ibid. 1823, p. 516.

§ Annales des Sciences Nat. tom. viii. p. 79. *et seq.* contain a translation of M. MAGNUS' paper, with a figure of the apparatus, &c. used; the original appeared in POGGENDORF's Journal, vol. xl. part 3.

I shall briefly mention the trials which I consider most conclusive, and which satisfied me in spite of an opposite pre-existing bias.

Vials provided with well-ground glass stoppers were filled with distilled water, deprived as much as possible of air by the air-pump; they were then placed under the receiver and kept in vacuo until all adhering air was removed; the stoppers were now introduced, all air being excluded, and they were instantly immersed in distilled water, which had been well boiled. Thus prepared they were taken to an adjoining slaughter-house, where they were filled with Sheep's blood in the following manner, without its coming in contact with the air. For venous blood the jugular vein was exposed; two ligatures were applied to it; the vein was divided between the two, and the upper part, slightly detached, was introduced into a prepared vial under water the instant the stopper was withdrawn, and laid open. The heavier blood proceeding from the vessel of course expelled the water; and when it was supposed to be all expelled the stopper was restored, and the vial was replaced in the water. In the instance of arterial blood it was collected in the same manner from the carotid artery. In some trials the blood was allowed to coagulate undisturbed; in others the fibrin was detached, and the liquidity of the blood preserved by agitating it, the instant the stopper was replaced, with some mercury, introduced with the distilled water, and equally deprived of adhering air.

In about half an hour from the abstraction of the blood, in every instance, it was subjected to the air-pump. The instant the stopper was withdrawn the vial was placed in a small receiver on the plate of the pump, and covered with a little larger receiver, and the air as soon as possible exhausted. No appearance of disengagement of gas was perceptible until the exhaustion was nearly complete; then it was sudden, sometimes considerable, even to overflowing, in the form of bubbles, and it continued some time. The results were not distinctly different, that I could perceive, whether venous or arterial blood was used; I am disposed to think, on the whole, that less air was disengaged from the arterial blood than from the venous.

When blood allowed to coagulate in the vessels was tried, the results varied a little, and appeared to me instructive. At first, on exhaustion, only a few particles of air were disengaged; no more, it might be supposed, than were derived from the contact of the end of the stopper. In two experiments such was the appearance for at least five minutes, conveying the idea that no air was extricated; then abruptly a bubble or film burst with some force, as was denoted by the scattering of the particles of blood; and a bubbling commenced and continued, rendering the indications of extrication of gas unquestionable. And in conformity with this result I may remark, I have never succeeded in obtaining indications of air in the blood in operating on it by the air-pump, if confined in a detached portion of vein, or in the heart of animals, the great vessels, previous to excision, having been tightly secured by ligature. In no instance of this kind have I witnessed any distention, such as occurs if air be admitted previous to the application of the ligatures: clearly indicative, it appears to

me, that a very slight compressing force is sufficient to confine the air in the blood, or rather, I should say, prevent its substance assuming the elastic state; and further, the probability, that the quantity of air so condensed is small.

I have stated that in the majority of instances the indications of the disengagement of air from blood in vacuo have been manifest. Exceptions, however, have occurred, and those clear and decisive; inducing me to believe that the quantity of air condensed in the blood is variable; that there are times when it is in excess, and times when in deficiency, and when totally wanting, connected with regularly changing states of the functional system. I hope on another occasion to be able to recur to this part of the subject, on which at present I have collected but a few facts. I may add, that such facts tend in part to reconcile some of the discrepancies referred to in the beginning of this section.

V. *Of the air or gases contained in the blood capable of being extricated by the air-pump.*

As already mentioned, M. MAGNUS has stated that these gases are carbonic acid, oxygen and azote in notable quantities. Taking the mean of ten of his experiments on the blood of the Horse and Calf (five on arterial and five on venous), the total quantity of the mixed gases he obtained was, in the instance of arterial blood, 10·4 per cent. per volume, and in that of venous 7·6 per cent.; the former consisting of about

6·5 carbonic acid,
2·4 oxygen,
1·5 azote;

the latter of

5·5 carbonic acid,
1·1 oxygen,
1·0 azote.

On a subject of so much importance, it is very desirable that the experiments of M. MAGNUS should be repeated and verified; for until this is done, considering the physiological history of the blood, it will be difficult to avoid doubt, and to depend on them with that degree of confidence which is justly due only to well-authenticated facts.

The ingenious apparatus employed by M. MAGNUS being difficult of construction, and not easily used excepting in a well-appointed laboratory, I must leave the repetition of his highly interesting experiments to those inquirers who are more happily situated than myself for engaging in them. On the present occasion I shall limit myself to the detail of some experiments instituted with a view of testing M. MAGNUS' general results.

As a solution of potassa has the property of absorbing carbonic acid gas, it follows that if mixed with the blood previous to being subjected to the air-pump, it will prove in some measure a test of the kind of air which the blood is capable of affording.

With this intent two vials were prepared, the same as before used, one filled with distilled water, the other with a weak aqueous solution of caustic potash, both care-

fully deprived of air by the air-pump. Observing the same precautions as before, a portion of venous blood from a Sheep was received into each of them. When less than half of the water and of the solution, as well as could be guessed, was expelled, the vials were closed with the glass stoppers belonging to them, and instantly immersed in water, and as soon as possible subjected to the air-pump. The results of exhaustion in the two instances was perfectly distinct. From the blood mixed with water gas was disengaged; there was a continued ascension of bubbles. From the blood mixed with the alkaline solution no gas was liberated, excepting a bubble or two, which might fairly be considered as entangled air derived from contact of the blood with the stopper.

A similar comparative trial was instituted with arterial blood of the Sheep, and the results also were similar and equally well marked.

Considered as test experiments, the first inference from these results is, that carbonic acid gas, or an air absorbable by a solution of potash, is disengaged both by venous and arterial blood in vacuo; and next, that no other gas is disengaged from either of them, neither oxygen nor azote, each of which is unabsorbable by the solution in question. I have repeated the experiment twice on the venous blood of Man, and twice on the venous blood of the Sheep, and twice on the arterial blood of the latter, without variation of results; and they are more to be depended on, as the alkali has the effect of preserving the blood liquid.

I may mention another method which I have employed as a test experiment; and first in relation to carbonic acid. If the blood contain a notable portion of this gas capable of being extricated by the air-pump, it necessarily follows, that when subjected to the action of the air-pump, and deprived as far as possible of its fixed air by this means, it will be capable of absorbing a larger quantity of carbonic acid than previous to the exhaustion. The following Table contains the results of three comparative trials on the venous blood of the Sheep, its fibrin separated or detached in the usual manner.

Table showing the absorption of carbonic acid gas by

Venous blood subjected to the air-pump.			Venous blood not subjected to the air-pump.		
Volume of blood used.	Volume of carbonic acid introduced.	Volume of gas absorbed.	Volume of blood used.	Volume of gas introduced.	Volume of gas absorbed.
27	46	38	27	47	37
27	45	39	27	45	39
27	46	35	27	45	34

Although I have thought it right to notice these results, and although they are in accordance with the preceding, I do not attach value to them, excepting as tending to show that the quantity of carbonic acid gas extracted by the air-pump, when the blood affords it in vacuo, is small.

The same mode of reasoning suggested comparative trials of the absorbent power

of arterial blood for oxygen, before and after exhaustion by the air-pump, as a further test experiment, whether arterial blood contain oxygen in a free state, that is, admitting of being extricated by the removal of atmospheric pressure. The result of this trial also has been negative; its power of absorbing oxygen has not appeared to be at all increased by exhaustion; this at least was the result of one experiment carefully conducted.

VI. *Is any oxygen contained in the blood not capable of being extricated by the air-pump?*

Before I was acquainted with the researches of M. MAGNUS I had instituted some experiments to endeavour to determine whether any oxygen in a free state, or in a condition approximating to that state, exists in the blood, and especially in the arterial, admitting of being detected by means of substances possessing a strong attraction for oxygen, or of being expelled by substances of greater solubility in blood than oxygen. Hydrogen, phosphuretted hydrogen, sulphuretted hydrogen, nitrous oxide and nitrous gas, it appeared advisable to try, as belonging to the first class of substances, and carbonic acid gas as belonging to the latter.

The results with hydrogen, sulphuretted hydrogen, nitrous oxide and carbonic acid gas, were of a negative kind. Neither using arterial blood, nor blood which had been agitated with oxygen, and which had absorbed or made to disappear a certain quantity of this gas, could I detect any indications of its presence either by combination or expulsion. In the instances of nitrous oxide and carbonic acid, however, it may be worthy of remark, that the blood which had been agitated with oxygen absorbed less of either of these gases than it did before it was so treated*.

The results with phosphuretted hydrogen, the spontaneously inflammable species, were of an ambiguous kind, not sufficiently clear to deduce from them any satisfactory conclusion. In one trial, serum of the venous blood of the Sheep absorbed nine per cent. of this gas; venous blood 11·3 per cent.; and arterial 5·8 per cent†.

The results with nitrous gas were of a different kind, and may be deserving of being specially noticed.

The blood used was that of the Sheep prepared in the usual manner. The experiments were made during the very cold weather which prevailed in the beginning of the year, and the difference of colour between the venous and arterial blood was very strongly marked.

* Nitrous oxide I find is absorbed in about the same proportion by venous and arterial blood, and by the serum of blood, and also in about the same proportion by water. Thus, at the temperature of 45° over mercury, using the blood and serum of the same animal (the Sheep), thirty-two measures of each absorbed twenty-two measures of this gas, and thirty-two of distilled water absorbed 21·5.

† Supposing the gas decomposed, the phosphorus uniting with the oxygen in the blood, the apparent smaller absorption by arterial than by venous blood is what might be expected, on the idea that the former kind of blood contains most oxygen.

1st. *On Arterial Blood.*

1. Fifty-three measures of this blood were agitated over mercury with forty-six measures of nitrous gas; there was a diminution of volume of seventeen measures.

2. Fifty-three measures of the same blood (another portion) were agitated with nine of oxygen; two measures were absorbed.

3. Fifty measures of this blood, so treated with oxygen, were agitated with forty-seven of nitrous gas; there was a diminution of twenty-two measures.

2nd. *On Venous Blood.*

1. Fifty-three measures of this blood were agitated with fifty of nitrous gas; there was a diminution of ten measures.

2. Fifty-three measures (another portion) were agitated with ten of oxygen; five measures were absorbed.

3. Fifty-one measures of blood so treated were agitated with forty-nine of nitrous gas; there was a diminution of seventeen measures.

The residual air in each instance was examined and was found to be a mixture of nitrous gas and azote without carbonic acid gas. The azote, it may be presumed, was introduced with the nitrous gas; it was in the same proportion as that which adulterated it, viz. about four per cent. That the residual air was free from carbonic acid was inferred from the circumstance, that in comparative experiments with and without addition of a portion of solution of caustic alkali, there was no difference in the proportion of nitrous gas absorbed: and it was corroborated by another circumstance, viz. that after the absorption of the nitrous gas, the blood was capable of absorbing seventy-five per cent of carbonic acid gas*; and further by the result that when nitrous gas is added not to saturation, the whole of it is absorbed†.

As regards the blood itself, the colour of both venous and arterial was altered; both were rendered darker and browner, as if a minute quantity of nitric acid had been added to them, a change long known to be occasioned by nitrous gas. In the degree of change there was however a difference; in the instance of arterial and of oxygenated arterial blood, it was more strongly marked than in that of the venous.

In conjunction with the unsuccessful attempts with the other gases already mentioned, do not the results just described indicate that a portion of oxygen exists in

* To some venous blood of a Sheep which absorbed 182 per cent. of carbonic acid gas, so much of a solution of pure hydrate of potash was added, that it absorbed 218 per cent. of the acid gas; seventeen measures of this blood with excess of alkali, agitated with fifty-one of nitrous gas, absorbed 5·5 measures; sixteen of the blood without the excess of alkali absorbed five measures: thirty-four measures of carbonic acid gas were added to the latter, the excess of nitrous gas being left in the tube; on agitation twelve measures of the carbonic acid were absorbed.

† In one experiment eighty-four measures of the venous blood of the Sheep were agitated with ten of nitrous gas over mercury; the whole of the gas was absorbed: twelve of oxygen were added; on agitation again there was no further absorption.

the blood, not capable of being extracted by the air-pump, and yet capable of entering into combination with nitrous gas, and which exists in largest proportion in arterial blood? Unless this conclusion is adopted, it must be supposed either that the nitrous gas which disappears is decomposed, or that it combines directly with the red particles; neither of which suppositions is well supported by facts. The circumstance that no azote is disengaged, is not favourable to the idea that the nitrous gas is decomposed; and the difference of effect in the instances of venous and arterial blood, and, after and before agitation with oxygen, is not in accordance with the notion of direct combination. I may mention another fact which seems to have the same bearing: serum, which does not absorb oxygen, I find also does not absorb nitrous gas, excepting in about the proportion in which water absorbs it: and, further, in corroboration, I may mention, that as blood putrefies, whether arterial or venous, its power of absorbing nitrous gas diminishes; and that it is also diminished by being agitated with phosphuretted hydrogen. Thus the arterial blood of a Sheep, which before agitation with phosphuretted hydrogen absorbed 45·3 per cent. of nitrous gas, after agitation with it absorbed 7·4 per cent. less; and, after it had become putrid, it absorbed twenty per cent. less. According to my observations, arterial blood does not lose its peculiar florid hue under the action of the air-pump. Is not this also in favour of the above inference, that a portion of oxygen is retained by the blood resisting extraction by the air-pump? I find also that when venous blood is agitated with oxygen and subjected to the air-pump, it, in like manner, retains its acquired florid vermilion hue, and likewise a power of absorbing an additional quantity of nitrous gas.

VII. *When oxygen is absorbed by the blood is there any production of heat?*

To endeavour to determine this point, of so much interest in connexion with the theory of animal heat, a very thin vial, of the capacity of eight liquid ounces, was selected and carefully enveloped in bad conducting substances, viz. several folds of flannel, of fine oiled paper, and of oiled cloth. Thus prepared, and a perforated cork being provided, holding a delicate thermometer, two cubic inches of mercury were introduced, and immediately after it was filled with venous blood, kept liquid as before described. The vial was now corked and shaken; the thermometer included was stationary at 45°. After five minutes that it was so stationary, the thermometer was withdrawn, the vial closed by another cork was transferred inverted to a mercurial bath, and $1\frac{1}{2}$ cubic inch of oxygen was introduced. The common cork was returned, and the vial was well agitated for about a minute; the thermometer was now introduced, it rose immediately to 46°, and continuing the agitation it rose further to 46·5, very nearly to 47°. This experiment was made on the 12th of last February on the blood of the Sheep.

On the following day a similar experiment was made on the venous blood of Man. The vial was filled with eleven cubic inches of this blood, its fibrin broken up in the usual manner, and with three cubic inches of mercury; the temperature of the blood

and mercury was 42·5, and the temperature was the same after the introduction of three cubic inches of oxygen. The temperature of the room being 47°, a fire having shortly before been lit, the vial was taken to an adjoining passage where the temperature of the air was 39°. Here the vial was well agitated, held in the hand with thick gloves on as an additional protection. After about three quarters of a minute the thermometer in the vial had risen a degree, viz. to 43·5.

On the 14th of the same month a third experiment was made on venous blood from the jugular vein of a Sheep. The vial was filled with 3·5 cubic inches of mercury and eleven cubic inches of blood. The thermometer in the bottle, left five minutes, was stationary at 49°; the temperature of the mercurial bath was 49°; the air of the room was 52°; a thermometer with its bulb moistened was 45°, which I mention because the outer covering of the vial was moistened with some blood which had overflowed. After three cubic inches and a half of oxygen had been introduced, before agitation, the thermometer was still 49°. The bottle was briskly shaken for about half a minute; now, on observing the thermometer, it was found at 50°; the vial was again agitated; there was no further increase of temperature: after ten minutes it had fallen to 49°.

I shall relate one experiment more, and that on arterial blood. It was made on the 14th of February, and in the same manner as those on the venous blood. Before and after the introduction of the oxygen, the blood, which was from the carotid artery of the Sheep, was 45°; after agitation with oxygen it rose to 45·5: this was done when the temperature of the air was 39°.

In a former part of this paper I proposed to recur to the question, Is the fixation of oxygen in the blood attended with the formation of carbonic acid gas? The change of colour accompanying the fixation of oxygen by the blood, so different from that produced by carbonic acid, and the effect of nitrous gas before and after, seem to be most in favour of the idea, that the oxygen, in the first instance, is simply absorbed, and that the heat evolved is merely the effect of its condensation; or, that if any of it enters into immediate union with the carbon, it is only a small part of the whole.

VIII. *Conclusion.*

Should the results detailed in the preceding pages be confirmed on repetition, they can hardly fail having some effect on the theory of respiration and animal heat.

As regards the former, they appear to me to tend to show that the lungs are absorbing and secreting, and perhaps exhaling organs, and that their peculiar function is to introduce oxygen into the blood and separate carbonic acid from the blood.

As regards animal heat, they appear to favour the idea, that it is owing, first, to the fixation or condensation of oxygen in the blood in the lungs in conversion from venous to arterial; and secondly, to the combinations into which it enters in the circulation in connexion with the different secretions and changes essential to animal life.

In illustration of what I imagine the secreting power of the lungs, I may mention the difference of effects in an instance of death by strangulation, and another by ex-

haustion of air from the lungs by the air-pump. A full-grown Guinea Pig was the subject of experiment in each. The one killed by strangulation died in about a minute after a cord had been drawn tightly round its neck; the other, placed on the plate of the air-pump and confined by a receiver just large enough to hold it, lived about five minutes after the exhaustion had been commenced, the pump the whole time having been worked rapidly. The bodies were immediately examined. The heart of the strangled animal was motionless; it was distended with dark blood; twelve measures of the blood, broken up and agitated with twenty-nine of carbonic acid gas, absorbed eighteen measures, or 150 per cent. The heart of the other Guinea Pig was also distended with blood, but of a less dark hue. Its auricles were feebly acting; the lungs were paler than in the former and more collapsed: ten measures of blood from the heart, broken up and agitated with fifty of carbonic acid gas, absorbed thirty-seven measures, or 370 per cent.!

Further, in illustration of this supposed secreting power of the lungs, I might adduce the condition of the blood in disease, and in instances in which I have examined it after death from disease, in the majority of which I have found the blood loaded with carbonic acid, as indicated both by the disengagement of this gas, when the blood was agitated with another gas, and by the comparatively small proportion of carbonic acid which the blood was capable of absorbing. This condition of the blood, in relation to carbonic acid, I believe to be one of great interest and importance, and capable, when further investigated, of throwing light on many obscure parts of pathology, and especially on the immediate cause of death, and that happy absence of pain in dying which is commonly witnessed.

As regards an exhaling power, which I suppose the lungs may possess, I conceive it may be exercised occasionally under peculiar circumstances—circumstances, in the first instance, favouring an accumulation of carbonic acid gas in the blood, as undue pressure of any kind, and, in the second instance, circumstances of a different nature, connected with the removal of undue pressure, admitting thereby the excess to pass off.

The view which I have alluded to relative to the production of animal heat, is, I believe, capable of explaining very many particulars of animal temperature in different classes of animals, and both during life, in health, and disease, and in a state of hybernation and after death. If correct, this it must necessarily do, theory being merely an expression of facts and truths in nature being perfectly consistent.

Fort Pitt, Chatham,
May 30*th*, 1838.

ERRATUM.

Page 288, line 28, *for* $\frac{114}{100}$ *read* $\frac{14}{100}$.

XV. *Researches in Embryology. First Series. By* Martin Barry, *M.D. F.R.S.E., Fellow of the Royal College of Physicians in Edinburgh. Communicated by* P. M. Roget, *M.D. Sec. R.S.*

Received June 20,—Read June 21, 1838.

It has been truly said, that "in all the sciences of observation, the great difficulty generally consists in taking the first steps." A hundred and fifty years have now elapsed since the celebrated Regner de Graaf, after a series of well-conducted observations, maintained that the ovum of the Mammalia must exist already formed in the ovary; an opinion which, after meeting with violent opposition, appears to have been nearly abandoned, and superseded by the notion countenanced by Haller, that the ovum was formed in the Fallopian tube out of a substance discharged from the ovary. A century after De Graaf had promulgated his opinions, Cruikshank arrived at the same conclusion, that the ovum was really formed in the ovary; but he sought it there in vain. Prevost and Dumas in 1824 obtained a glimpse of something that must have been the ovum in that organ; Von Baer in 1827 found and recognised it there.

This important discovery of Baer formed an epoch in the history of development; but it was a "first step," and the object one of extreme minuteness. It was therefore not surprising if the excellent discoverer did not see or justly estimate all that appertained thereto; and he said himself "there remains yet many a thing that will become a prize" for others.

Von Baer for instance did not see the germinal vesicle contained within the mammiferous ovum; he saw no more than a transparent space. This, however, was an oversight of the first importance, because that which gives peculiar interest to the germinal vesicle is the fact, now generally acknowledged, that it is the most essential portion of the ovum; and besides, as in the following pages I shall have to show, this structure and its contents are the earliest that appear in the order of formation. Overlooking the germinal vesicle in the Mammalia, Von Baer supposed the ovum itself to represent, in that class of animals, the germinal vesicle of Birds†. Other analogies, and they are not few, which he based on this were consequently erroneous.

I have thought it proper to make these introductory remarks, because of the neces-

† Professor Baer called the vesicle he discovered in the ovary, not the *ovum* but the "*ovulum.*" If, however, it can be made to appear extremely probable that the chorion or external membrane of the ovum of the uterus is a primitive part of the ovarian vesicle of Baer, it is perhaps better to call the latter an *ovum*, as will be done in this memoir.

sity that lies before me, in offering the present contribution in embryology, for pointing out several other structures connected with the discovery of BAER, the nature of which I believe this author to have mistaken, and some that appear to have escaped his notice. This I shall do with great deference, asking his re-inspection of the parts, and respectfully soliciting an examination of them by other physiologists who have been engaged in researches on the early stages of the ovum of the Mammalia, especially Professors PURKINJE, KRAUSE and COSTE; but more particularly Professor VALENTIN and my valued friend Professor RUDOLPH WAGNER.

In the course of this investigation, which occupied many months, the number of individual animals examined was very large, furnishing me an opportunity for verifying most of the facts by repeated observation. The examinations were generally made very soon after death; and the drawings having been all taken by myself, I have it in my power to say that their fidelity may be relied upon†.

In the year 1837, preparatory to the commencement of my own researches, I spent some time in Germany for the purpose of becoming acquainted with the known facts on animal development and other objects of microscopic research; and cannot omit this opportunity of gratefully acknowledging the facilities afforded me on that occasion by my honoured friends Professors JOHANN MÜLLER, EHRENBERG, RUDOLPH WAGNER, and Dr. TH. SCHWANN. The microscope I use is an achromatic, since made for me by SCHIEK of Berlin, and similar in all respects to the instrument employed by Professor EHRENBERG.

It may not be improper in the first place to furnish an idea of what has been already published on some branches of the subject; for it is one to which the attention of physiologists in this country has scarcely begun to be directed.

VALENTIN gives the following comprehensive description of the vesicles of GRAAF:—

"In the ovary of each of the Mammalia, there is found a greater or less number of spherical pellucid vesicles, the so-called *folliculi Graafiani*, (Graafian vesicles,) the greater part of which are situated near the surface of the organ. Their size is very different, as well in different animals as in the same ovary of the same animal, since the older ones are sometimes four to five times, as in Rabbits, Dogs, Cats; sometimes eight to ten times, as in Man; sometimes ten to twenty times, as in ruminating animals; sometimes thirty to fifty times, and still more, as in the Swine; larger than those recently formed, and this, too, independently of their increase in volume after impregnation. When situated close to the surface, they are covered by the peritoneum alone; when deeper, by the latter, and by the fibrous tissue of the ovary, and closely invested by a net-work of vessels, between which there is a granular membrane. They are everywhere closed, without a trace of processes, but in-

† Some of the figures to be seen to advantage should be viewed with a large lens.

timately united with the substance of the ovary, so that in general it is not so very easy to dissect the vesicle freely out on all sides without laceration †."

Plate VII. fig. 62‡ presents the parts which BAER considers to enter into the formation of a Graafian vesicle. For an explanation of this figure I refer to the description of the plate. It appears to me however, as I shall endeavour presently to show, that a "Graafian vesicle" is *an ovisac that has acquired a covering proper to itself*, and as such the term will be used in the present paper.

The figure just referred to, exhibits the situation of the "ovulum" (ovum) of Mammalia, *according to* BAER, surrounded by its so-called "disc" within the Graafian vesicle. It will be shown in this memoir that there is no structure in the Graafian vesicle of a *discoid* form; and the error of BAER in mistaking the "ovulum" (ovum) of Mammalia for a part corresponding to the germinal vesicle of other animals, appears to be attributable in part to his misconception of the situation of the "ovulum" (ovum) in reference to its surrounding granules.

PURKINJE§ questioned the analogy which BAER had imagined between his "ovulum" (ovum) of the Mammalia and the germinal vesicle of other animals; and the discovery at length by COSTE in France, and by VALENTIN and BERNHARDT in Germany, of a germinal vesicle in Mammals, showed the justness of PURKINJE's reasoning, increasing the analogy between the bodies expelled respectively from the ovaries of Mammalia and those of other vertebrated animals. It will presently be shown that a perfect analogy does not exist between these bodies.

One of the last additions made to our knowledge in embryology, exceeding in minuteness all the rest, is the discovery by Professor RUDOLPH WAGNER||, on the internal surface of the germinal vesicle, of the *macula germinativa*, or germinal (germinative) spot.

The object of this memoir is to add some discoveries of my own on the early stages of the ovum, not only of the Mammalia, but of the other Vertebrata. This cannot be done except by means of details both minute and, I fear, fatiguing; but perhaps I may venture to hope for the reader's indulgence when he recollects the precision requisite in treating a question comparatively new in the history of physiology. The perusal would, however, be much facilitated by occasional reference to the following tabular Synopsis of the parts to be described. It might also be advantageous to examine the plates (V. to VIII.) in connexion with this table; for as the same letters denote the same objects in all the figures, and as these letters have been introduced

† Translation from the German, in the Edinburgh Medical and Surgical Journal, No. 127; which contains the early part of Professor VALENTIN's elaborate work "Handbuch der Entwickelungsgeschichte des Menschen mit vergleichender Rücksicht der Entwickelung der Säugethiere und Vögel," Berlin, 1835.

‡ Taken from BAER, Lettre sur la Formation de l'Œuf, translated by BRESCHET, Paris, 1829. fig. ix. (I have not the original at hand.)

§ Encyclopädisches Wörterbuch, Band x. Artikel "Ei," pp. 124—128, Berlin, 1834.

|| Lehrbuch der Vergleichenden Anatomie, S. 351, Leipzig, 1834–5. Edinburgh Medical and Surgical Journal, No. 127. Prodromus Historiæ Generationis Hominis atque Animalium, Lipsiæ, 1836.

into the table, a reference to the more particular explanation of the plates might thus in a great measure be spared.

Synopsis showing the Order of Formation and the relative Position of the several Parts of the unimpregnated Ovum and the Graafian Vesicle of Mammalia, according to the author's observations.

First.	Second.	Third.	Fourth.	Fifth.	Sixth.		Seventh.
Fluid, *a*.	Fluid.	Fluid, *a*.	Fluid, *a*.	Fluid, *a*.	Fluid, *a*.		Fluid, *a*.
	Germ. spot.	Germ. spot, *b*.	Germ. spot, *b*.	Germ. spot, *b*.	Germ. spot, *b*.		Germ. spot, *b*.
Germ. vesicle, *c*.	Germ. ves.	Germ. ves. *c*.	Germ. ves. *c*. Oil-like globules (*).	Germ. ves. *c*.	Germ. ves. *c*.		Germ. ves. *c*.
Granules (*g*) and oil-like globules (*) †.	Granules (*g*) and oil-like globules(*).	Granules (*g*) and Fluid.	Granules (*g*) and Fluid.	Oil-like globules (*), Minute gran., and Fluid.	Yelk, *d*.		Yelk, *d*.
				Memb. vit. *e*.	Memb. vitelli.	At this period the ovum is conveyed from the centre of the Graafian vesicle to its periphery.	Memb. vitelli.
				Chorion, *f*.	Chorion, *f*.		Chorion, *f*.
	Ovisac, h.	*Ovisac, h*.	*Ovisac, h*.				
				Granules (*g*) and Fluid.	*Tun. gran.* g^1.		*Tun. gran.* (g^1) *and appendages.*
				Ovisac, h.	*Retinac.* g^2.		*Retinac.* (g^2) *and Memb.*
					Fluid.		Fluid.
					Memb. gran. g^3.		Memb. gran. g^3.
					Ovisac, h. *Tunic, i.* } = Graaf. ves. *h i*.		Graaf. ves. *h i*.
Plate V. figs. 1, 2, 3.	Plate V. fig. 9. &c.	Plate V. fig. 10. &c.	Plate V. fig. 14. &c.	Plate V. fig. 17.	Plate VII. fig. 50. &c.		Plate VII. fig. 55. &c.

The Table viewed *from left to right*, presents successive stages of Formation. The parts inserted in the same Column exist at the same time. Small print denotes such parts as, though existing, are obscured.

Viewed *from above downwards*, the Table shows the order of Position of the parts, proceeding outwards from the centre of the ovum; the innermost being marked *a*, the next *b*, the third *c*, and so on.

Italics are used in cases where the part has not been hitherto described, or where the name employed is new.

† The term "oil-like," occurs frequently in this memoir. It is not intended to describe the nature of the globules to which it is applied, but simply their appearance.

PART I.

On the origin and structure of the true Ovisac, a vesicle common to all vertebrated animals, but hitherto described as the inner membrane of the "Folliculus Graafianus" in Mammalia, and by some authors denominated the "chorion" in other Vertebrata; on the real nature of the "Folliculus Graafianus," and its relation to the calyx of the Bird; on the Germinal Vesicle and its contents, as the most primitive portion of the ovum; on the order of formation of the several other parts of the Ovarian Ovum; and on the true Chorion of Mammalia as a structure superadded within the ovary.

1. The early structure of the ovisac in the Mammalia may be seen in either quite young animals, or in those that have lately reached the state of puberty, when this vesicle and its contents are in the full vigour of formation. To find it in a young animal, I am in the practice of placing under the microscope thin slices cut from the surface of the ovary; or in animals after puberty, the outer portion of a large Graafian vesicle. Compression is of course required, but it should be very gradually applied. Great pressure is rarely wanted, and for the most part very undesirable.

The Ovisac of the Mammalia.

2. This vesicle originates in the proper substance (*stroma*) of the ovary†. Its general appearance when first formed is as a minute, pellucid, often yellowish sac, having an elliptic form, and plaits or folds in its contour (Plate V. figs. 4. to 12. *h.*). The ovisacs are found in groups, and those belonging to the same group are often of nearly the same size (figs. 4. 10. *h.*), though this is by no means constantly the case. Sometimes a group occupies more than the whole field of view; some of the ovisacs, more deeply imbedded than the rest, having the appearance of obscure pellucid spaces, others being superficial and distinct (fig. 4. *h.*). Could a fragment of amygdaloidal rock be made translucent and its amygdaloidal portions luminous, it would present an appearance not unlike that of a collection of minute ovisacs in the parietes of a Graafian vesicle from one of the Ruminantia. In Carnivora, such as the Dog and Cat, I have met with them having less of interstitial substance, innumerably heaped together (Plate V. figs. 6. 10., Plate VIII. fig. 68. *h.*). The surface of an ovary presents to the naked eye perhaps ten, twenty, or fifty Graafian vesicles, while the microscope discloses myriads; so that some parts bear no mean resemblance to the roe of fishes.

3. The ovisac at its origin, as just said, has an elliptic form. As its size advances it becomes more spherical, and is often met with somewhat tapered at one end (Plate VIII. fig. 74. *h.*). When it appears round in an early stage, this is perhaps owing to its being transversely situated under the compressor.

† It appears sometimes to originate in the parietes of a Graafian vesicle. Ovisacs met with in this situation will be more particularly referred to hereafter as *parasitic* ovisacs.

4. Its size when first formed is exceedingly minute. I have found it in several orders of Mammalia measuring not more than the 50th of a Paris line † in length. Such is the case, for instance, in the Ox (Plate V. fig. 4. *h.*); so that a cubic inch would contain upwards of two hundred millions, not merely of the elements of the ovum, but of the *ovisacs* of this animal. The minuteness of these vesicles indeed is almost incredible. In the Dog I have seen them measuring only the 100th of a Paris line ‡ (Plate V. fig. 9. *h.*), thus little more than one third the long diameter of a blood-granule (red particle) of the *Proteus anguinus* §.

5. The ovisac is more or less pellucid according to its size; being most so in the early stages of formation, and becoming merely translucent as development advances. This is partly owing to the gradual addition of an external covering or tunic, to be hereafter more particularly mentioned (24. 25.). It continues however in all its stages more translucent than the substance in which it lies.

Cavity in which the Ovisac is often found.

6. More particularly considered, the situation of the ovisac in its early state is often found to be a cavity (Plate VIII. fig. 69.), sometimes, as already said, in the proper substance (*stroma*) of the ovary, and sometimes in the parietes of a Graafian vesicle. It lies loose in, and unconnected with the walls of, its containing cavity. That it does so is shown in the figure just referred to (fig. 69.), which presents an ovum, *f*, escaped from the ovisac, *h*, and lying external to the latter in the cavity. (This change in the situation of the ovum was seen to take place on the ovisac being burst under the microscope by means of the compressor; when the ovum became squeezed into an elliptic form. This has repeatedly occurred ||.) The cavity in which the ovisac lies, may sometimes be found after the latter has been pressed out of it.

The proper membrane of the Ovisac in Mammalia.

7. In ovisacs of the minutest size this membrane is perfectly transparent; yet in and near to its contour it looks as though it were "concentrically lamellar" ¶ (Plate V. figs. 9. 11. 12. *h.*). I apprehend this appearance to arise from plaits or folds occurring in the membrane under pressure even at that early period, and indicating great susceptibility of distention. Its thickness is relatively very considerable in the smaller

† That is, about the 562nd of an English inch. See the table of measurements (118.), for a simple mode of reducing the fraction of a French line (‴) into the fraction of an English inch.

‡ = $\frac{1}{1123}$ of an English inch.

§ Through the kindness of Professor R. WAGNER I possess a living specimen of this animal, and have repeatedly confirmed the observation first made by him, that the red particles of its blood measure in some instances $\frac{1}{30}$ of a Paris line in length, and that they are visible with the naked eye, being larger than those of any other animal the blood of which has been examined.

|| The ovum in this instance was ruptured before escaping, and left its germinal vesicle, *c*, behind.

¶ The appearance here referred to has been observed by R. WAGNER, who uses the above expression in describing what he considers minute *Graafian vesicles* (Beiträge, &c., S. 28.).

ovisacs. The membrane of one of these, measuring in its long diameter $\frac{1}{50}$th of a Paris line†, had a thickness of $\frac{1}{600}$th of a line‡. When somewhat advanced in size, however, the thickness is sometimes relatively as well as absolutely greater. Yet so transparent is this membrane, that even the form of granules contained in its cavity may be tolerably well observed from the exterior (Plate V. figs. 9. 10. 11. 12. 17. *g.*); the degree of transparency varying with the pressure applied: and when great pressure has been removed, the previous state in this respect returns, and the vesicle resumes more or less completely its natural size, which indicates elasticity as well as susceptibility of distention. This membrane has a kind of undulating surface, presenting myriads of depressions or cells, to which is referrible the plaited or folded appearance of the contour, produced by pressure. Neither fibres nor granules being found in the texture of this membrane, it is probably without organization.

The peculiar Granules of the Ovisac.

8. The only objects which in general are *visible* (12.) in the ovisac when the latter is first formed, are a few granules (Plate V. figs. 8. 9. 11. 12. *g.*). These granules deserve a particular description, from their peculiar appearance, from their entering into the formation of several important structures to be described in Part II. of this memoir, and from their being very intimately connected with the evolution of the ovum from its early appearance as the germinal vesicle (14—22.), until as a mature ovum it has passed into the Fallopian tube.

9. These granules are elliptical or ellipsoidal, sometimes nearly round, and generally flattened (Plate VIII. fig. 73. *g.*). When lying closely together, their form becomes by pressure polyhedrous (Plate VI. fig. 44. g^1.). They are exceedingly transparent, yet often punctate, which latter appearance seems sometimes to arise from the presence of very minute oil-like globules. They present, with more or less distinctness, a nucleus; and I have met with two nuclei in a single granule. In the substance of the nucleus, when highly magnified, there is observable a point still darker (Plate VIII. fig. 73.). These peculiar granules sometimes disappear, apparently by liquefaction; preparatory to which change I have observed them to become more spherical and brightly pellucid, seeming to contain a fluid in their interior (Plate VI. fig. 44. g^1.). In size I have found them to vary from about the 400th to the 100th of a Paris line§, but they are often about the 200th of a Paris line‖ in length. As viewed with reflected light, they appear greyish white in colour. Water dissolves them.

10. This is a general description of these very peculiar granules. In some orders of the Mammalia I have found them more regularly elliptic or more round than in others; in birds I have thought them less regular in form; in certain animals they have a decided tinge of yellow: but in these respects, as well as in their punctate or subgranulous appearance, and in their stages of liquefaction, their condition

† = $\frac{1}{562}$ of an English inch.

‡ = $\frac{1}{6750}$ of an English inch.

§ = $\frac{1}{4500}$ to the $\frac{1}{1125}$ of an English inch.

‖ = $\frac{1}{2250}$ of an English inch.

differs at different periods in the same animal. Yet notwithstanding these varieties there is always found a general resemblance, so that when once examined these granules cannot fail to be recognised†.

The Fluid contained in the Ovisac.

11. This fluid is pellucid, often yellowish, partially coagulable, and is generally considered to be albuminous. It contains a large quantity of the peculiar granules just described, and a varying quantity of oil-like globules. This fluid having been already described by others‡ I need not add more respecting it.

On the Germinal Vesicle and its contents as the most primitive portion of the Ovum.

12. It has appeared desirable to give a description of the ovisac the first place in this memoir, but there is a portion of the ovum which exists before it. Each of the many ovisacs already referred to, such as those in Plate V. fig. 4. *h.*, and even the minutest of those in fig. 9. *h.*, probably contained, besides the peculiar granules visible (8.) in their interior, a concealed part, which, supposed to be the most important, also appears to be the most primitive element of the ovum.

13. Purkinje, the first discoverer of the germinal vesicle in any animal (the Bird), having observed that its relative size was greater, the more minute its containing ovum, has expressed the opinion that it is the first part of the ovum formed§. Baer, the discoverer of this vesicle in other oviparous Vertebrata, as well as in Mollusca, Annelida, Crustacea, and Insects ||, believes that he observed its formation in animals belonging to the two first-mentioned classes to precede the evolution of the ovum¶. R. Wagner has described and figured the posterior extremity of the oviduct in insects (*Acheta campestris*)†† as filled with little besides germinal vesicles, each of which contains a spot; and in a later publication‡‡ he has shown similar objects in one of the Libellulæ (*Agrion*), and also in the freshwater Beetle (*Dyticus marginalis*). These observations of Baer and Wagner I have been enabled to extend certainly to two, and I think to three classes of the Vertebrata.

14. Plate V. fig. 1., taken from the Rabbit, presents vesicles, *c*, containing fluid, each of which has its own envelope, consisting of the peculiar granules, *g*, just described (8.). These vesicles, of which a considerable number was seen in the same field of view, were for the most part elliptical, some of them nearly globular, their long diameter varying from the 150th to the 50th of a Paris line§§. The contained

† Several writers have made mention of these granules, but an adequate description of them I do not find to have been given.

‡ Baer, Lettre, &c., pp. 18, 19. Valentin, Handbuch der Entwickelungsgeschichte, &c., S. 15, 16. Bernhardt, Symbolæ ad Ovi Mammalium Historiam ante Prægnationem, pp. 10, 11; Vratislaviæ, 1834.

§ Encyclopädisches Wörterbuch, Band x. S. cxi.

|| Burdach, Die Physiologie als Erfahrungswissenschaft, Band i. § 63. 1835.

¶ Lettre, &c., p. 21.

†† Prodromus, &c., fig. xviii. *a.*

‡‡ Beiträge, &c., Tab. ii. figs. 1 to 6.

§§ = $\frac{1}{1687}$ to $\frac{1}{562}$ of an English inch.

fluid did not appear perfectly pellucid, but presented an indistinct cloudiness. In one of the larger vesicles there was observed a well-marked spot (fig. 1. *b*.). The envelope of granules, *g*, was in all instances elliptical, its long diameter being about three times that of the vesicle it surrounded.

15. In the same Plate, figs. 18 and 19, from the Pigeon, are seen vesicles of the same kind containing fluid, some of them surrounded by oil-like globules, and others by granules of the same appearance as those in fig. 1, having mixed with them a few oil-like globules; the chief difference being that here the form of the whole object is not quite so regular as that of those in fig. 1. It will be observed that in fig. 18. the vesicles are surrounded by oil-like globules only (*); the envelope of peculiar granules seems to be subsequently added (fig. 19. *g*.).

16. In figs. 2. and 3, *g*, are presented elliptic masses of the same kind of peculiar granules. One of these masses (fig. 2. *g*.) measured $\frac{1}{75}$th, and the other (fig. 3. *g*.) $\frac{1}{50}$th of a line in length. These objects, met with in the Cat, appear to have been of the same nature as those just described from the Rabbit and the Pigeon, with this difference, that here the contained vesicle was not seen; the cause of which is perhaps referrible to oil-like globules having been mixed in large quantity with the peculiar granules. See figs. 2. and 3 (*). That these elliptic masses from the Cat (figs. 2. and 3.) corresponded to those from the Rabbit and the Pigeon, and that each of them contained a vesicle, is not indeed demonstrable; but their form, their size, and the nature of the granules render this extremely probable. I should add, as another reason for this opinion, that *patches* of these elliptic masses (figs. 2. and 3.) have been met with, sometimes in the state *g*, and sometimes in that marked (*). Similar objects have been seen in the Hog†.

17. I have found distinct vesicles in the Salmon, measuring in diameter the 50th of a Paris line‡, and less (Plate VI. fig. 30. *c*.), apparently corresponding to those from the Rabbit (Plate V. fig. 1. *c*.) and Pigeon (figs. 18. and 19. *c*.), but having more obscure contents§, and being destitute of a regularly formed envelope of granules.

18. I return to figs. 1, 18, and 19, Plate V., from the Rabbit and the Pigeon, and shall not refer to any besides these in drawing my conclusions. After an attentive examination of these objects, I do not think it hazardous to express my belief that they present one of the earliest stages in the formation of the ovum, the enveloped sac (*c*) being, as I suppose, the *germinal vesicle* itself.

19. The long diameter of this vesicle (*c*) in fig. 1, the stage now under consideration, varies, as already said, from the 150th to the 50th of a Paris line. It is not likely

† In one of the Mammalia I have seen vesicles filled with fluid, and apparently of the same kind as those in fig. 1, but without any peripheral accumulation either of granules or oil-like globules. These vesicles, however, were immediately lost from the field of view, and I have not since observed any in the same state.

‡ = $\frac{1}{562}$ of an English inch.

§ Which consisted of a fluid and minute oil-like globules, having a yellowish colour. Compare the vesicles in this figure with the vesicles (*c*) in fig. 35. from the same animal.

One of the vesicles (*c*) in fig. 30. has another vesicle external to it. (See Explanation of the Plates.)

that so great a difference in magnitude as this in the same animal should be permanent; and if this be admitted, it follows that after formation the germinal vesicle increases in its size. It may therefore when first formed be much minuter than the smallest of those which I have figured.

20. I find that the germinal vesicle of a mature ovum of the Rabbit does not generally exceed the 50th of a Paris line† in diameter; and this being just the size of the largest of those from the same animal in fig. 1, it seems probable that in the Rabbit‡ the vesicle does not increase much in magnitude after this early period.

21. It is proper to state, that in certain animals I have met with *ovisacs* much minuter than some of the objects (still uncovered by an ovisac) in fig. 1. This admits of explanation by supposing either that the germinal vesicle is much *smaller*, that its granulous envelope is much *thinner*, or that the period of formation of the ovisac, in reference to the size of the germinal vesicle, is much *earlier* in some animals than in others. There may also be a difference in different animals in each of these respects.

22. From the observations of Professors Baer and R. Wagner in invertebrated animals, and those now recorded from researches in two classes of the Vertebrata, I think I am warranted in concluding that the germinal vesicle and its contents constitute throughout the animal kingdom the most primitive portion of the ovum. In Birds and Mammalia the succeeding process appears to consist in the accumulation around the germinal vesicle of oil-like globules and peculiar granules. See the first column in the Table, page 304.

The manner of Origin of the Ovisac.

23. Around the elliptic envelope of granules and oil-like globules just described as proper to the germinal vesicle of Mammalia and Birds in an early stage, I find that there is formed a membrane, seen in Plate V. fig. 20. *h*. If the contents of this membrane, which is still incomplete, be compared with the granules (*g*) in fig. 19, these objects will be found to be the same; in farther proof of which it may be added that they were met with, as well as those in fig. 18, all lying together§. The membrane, *h*, is shown completely formed in nearly all the other figures of the same Plate. This is the membrane which I have already described in Mammalia, and intend describing in other Vertebrata, as the *ovisac*. See the second column in the Table, page 304.

The order of Formation of the several parts of the Ovarian Ovum.

24. After the formation of the ovisac, the germinal vesicle, as already said (12.), is generally for a short time concealed. This is perhaps partly referrible to minute oil-like globules being mixed with the peculiar granules of the ovisac, and causing great refraction. Liquefaction, however, of some of the granules appears to take place, or a fluid from some other source is added, and then the germinal vesicle is seen in or

† = $\frac{1}{564}$ of an English inch. ‡ And probably in Mammalia generally.

§ All of these objects, moreover, were lying among newly-formed ovisacs.

near the centre of the ovisac. The germinal vesicle is here surrounded by the peculiar granules of the ovisac, either immediately† (Plate V. fig. 10. *c*.), or having minute oil-like globules interposed‡ (fig. 14. and 15. (*)). The latter, with a pellucid fluid, indicate the incipient formation of the yelk around the germinal vesicle§. The oil-like globules accumulate (fig. 15. (*)), and minute opake granules gradually present themselves among them. When the formation of the yelk has thus proceeded to a certain stage, two membranes are seen surrounding it; one of which is the proper membrane of the yelk, *membrana vitelli* (49.), and the other, more external, is the true *chorion*‖ (52.) (fig. 17. *e*. and *f*.). The ovum is seen with great distinctness through the transparent membrane of the ovisac, and it is thus possible to follow its several stages of formation. Subsequently, a covering or tunic, consisting of a kind of dense cellular tissue susceptible of becoming highly vascular, and closely connected with the surrounding stroma, is gradually formed upon the outer surface of the ovisac, which previously in a high degree transparent, now becomes translucent only.

The Folliculus Graafianus, or Graafian Vesicle.

25. From the union of the ovisac with the covering or tunic now mentioned, I apprehend it is that there results what has been called a *Graafian vesicle*; and it appears to me that the "Couche *interne* de la capsule de la vésicule de De Graaf" of Baer (Plate VII. fig. 62. *h*.) is in reality the previously independent *ovisac*, while the "couche *externe*"¶ of this author (Plate VII. fig. 62. *i*.) is the *covering* or *tunic* of the ovisac above mentioned.

26. Professor Baer has expressed his belief that the formation of the "ovulum" (ovum) precedes that of the Graafian vesicle; but he adds, "Je doute qu'il soit jamais possible à l'homme de s'en convaincre par l'observation††." I trust that the facts mentioned in the preceding pages, and illustrated in Plate V., will suffice to show how far this eminent naturalist was right in his conjecture as to the priority in formation of the ovum; and that they will also serve to demonstrate the possibility (questioned by Baer) of determining this point *by observation*.

† See the third column in the Table, page 304. ‡ See the fourth column in the Table, page 304.

§ The germinal vesicle is thus in or near the centre of the yelk when the latter is first formed. This vesicle subsequently passes to the surface of the yelk.

T. W. Jones (London and Edinburgh Philosophical Magazine, No. 39, Sept. 1835, p. 209.) mentions the germinal vesicle as "having on one side a small elevation, which, projecting among the grains composing the granular sac, fixes the vesicle in its place." I have never observed this "small elevation," nor do I find any mention to have been made of it by either Coste or Valentin, who had previously described the germinal vesicle in Mammalia, or by those who have since written on it.

‖ See the fifth column in the Table, page 304.

¶ Von Baer says of this "couche externe," "Elle reçoit des vaisseaux, les contient dans son intérieur, et leurs extrémités vont dans la couche suivante" ("couche interne"). Lettre, &c., p. 17.

†† Lettre, &c., pp. 20, 21.

The Ovisac of Birds ("Chorion" of Authors) compared with the Ovisac of Mammals.

27. The ovarian calyx of the Bird, if deprived of its peritoneal investment, and what there is of the parenchyma† of the ovary, would, I apprehend, present *a structure analogous to the Graafian vesicle of Mammals*, that is, *a structure consisting of an ovisac that has acquired a proper (and in the later stages a highly vascular) covering.* Yet the ovisac of Birds has been mistaken for a vesicle corresponding to the *chorion* of Mammals, and accordingly it has been called the "chorion." The impropriety of this designation will appear evident if the vesicle in question be followed through its several stages of formation, which I do not find to have been hitherto attempted. For this purpose I need not do more than refer to the several figures.

28. Plate V. fig. 22. Ovisacs of the Pigeon, $\frac{1}{33}'''$ to $\frac{1}{25}'''$. Germinal vesicle (*c*) in the centre, surrounded by oil-like globules (*), the incipient yelk. Compare with fig. 15. from the Hog.

29. Plate V. fig. 23. Ovisac of the Common Fowl, $\frac{1}{20}'''$, with similar contents. Compare with fig. 15. from the Hog.

30. Plate V. fig. 24. Ovisac of the common Fowl, $\frac{1}{17}'''$. Yelk (*d*) just formed, but the membrana vitelli not yet visible.

31. Plate V. fig. 25. Ovisac of the common Fowl, $\frac{1}{14}'''$. Membrana vitelli (*e*) now seen. Compare with fig. 16. from the Hog.

32. Plate V. fig. 26. Ovisac of the common Fowl in a stage much more advanced; *g*, peculiar granules (8.). The yelk (*d*) has become in some degree opake. The ovisac has now a proper covering or tunic, and presents by this addition a structure (capsule) (*h i*) analogous to the Graafian vesicle of Mammals. The quantity of yelk in Birds is very large; hence that portion of the ovary which contains the structure now described (as analogous to the Graafian vesicle of Mammals) becomes pendent, and now the united coverings of the yelk-ball, viz. the ovisac, its proper tunic, the parenchyma of the ovary, and the peritoneal investment, are together called the *calyx*, which is connected with the ovary by a pedicle‡.

33. Plate VIII. fig. 76. Ovisac of the Pigeon, with its vascular covering or tunic. The yelk has escaped, and large granules, essentially the same as those in the ovisac of Mammalia (8.), are seen slowly passing out.

† This term is here intended to include the "*capsula propria*" of authors, for the same reason as that which influenced Baer in declining to admit any distinction, except a difference in density, between his *stroma* and the so-called "*tunica albuginea*" in the ovary of Mammals.

‡ When a minute incipient capsule is crushed under the microscope, the yelk is observed to escape into the pedicle. If the ideal section (Plate VI. fig. $38\frac{1}{2}$.) be referred to, it will be seen that the yelk, in taking this direction, ruptures the parictes of its containing cavity at a point where it meets with the least resistance. It ruptures, not the whole thickness of the capsule, but its internal membrane only, formerly the ovisac. (In this instance (Plate V. fig. 26.) the yelk is seen to have subsequently ruptured also the pedicle itself, and appears escaping through the opening.)

The Ovisac of Amphibia and Fishes ("Chorion" of Authors) compared with the Ovisac of Mammalia.

34. The ovisac of Amphibia, and that of Fishes, improperly called the "chorion," first presents itself in a very similar condition to the ovisacs of Mammalia and Birds, viz. *size*, the 50th of a Paris line† and less; *form*, generally elliptical or ellipsoidal; *contour*, in plaits or folds. The chief difference consists in the fluid of the ovisac not generally (though in some instances) presenting the peculiar granules (8.) described as occurring in Mammalia and Birds.

35. Plate VI. fig. 31. Ovisacs of the Cod, $\frac{1}{40}'''$ to $\frac{1}{10}'''$. Contour in folds. Germinal vesicle (*c*) in the centre, with its numerous germinal spots (*b*) ‡.

36. Plate VI. fig. 27. Ovisac of the common Frog, ("chorion" of R. Wagner§, from whom I take this figure,) $\frac{1}{50}'''$: *c.* germinal vesicle; *b.* germinal spots (many ‡); *e.* membrana vitelli, yelk pellucid.

37. Plate VI. fig. 32. Ovisacs of the Whiting, $\frac{1}{7}'''$ to $\frac{1}{5}'''$. There is a trace of yelk (*d*), with something like the peculiar granules (*g*) of the ovisac in Mammalia and Birds in the fluid of the ovisac. Compare with fig. 10. Plate V. from the Cat, and fig. 21. Plate V. from the Pigeon.

38. Plate VI. fig. 33. Ovisacs of the Haddock. Yelk (*d*) circumscribed by its proper, and here very delicate membrane.

39. Plate VI. fig. 27½. Ovisac|| of the *Coluber natrix*: *b.* germinal spot (single‡); *c.* germinal vesicle; *d.* yelk; *e.* membrana vitelli.

40. Plate VI. fig. 28. Ovisacs of the common Frog, $\frac{1}{5}'''$: *b.* germinal spot; *c.* germinal vesicle; *e.* membrana vitelli; *h.* proper membrane of the ovisac; *d'.* a spot which I always find on the internal surface of the membrana vitelli of the Frog in ovisacs of about this size. This spot does not appear to have been hitherto described. It is generally elliptic, rarely round, has a well-defined contour, and is perhaps slightly lenticular in form. In this instance it measured $\frac{1}{25}'''$ in length, and is often of about the same size. It appears to be composed of granules.

41. Plate VI. fig. 34. Ovisac of the Turbot, $\frac{1}{10}'''$: *e*, membrana vitelli now visible; *d.* yelk, with oil-like globules (*) around the germinal vesicle. Compare with fig. 25. Plate V. from the common Fowl, and fig. 16. Plate V. from the Hog.

42. Plate VI. fig. 35. Ovisacs of the Salmon, $\frac{1}{10}'''$ to $\frac{1}{8}'''$, in nearly the same stage of formation as the ovisac of the Turbot, fig. 34. At *g* are seen some of the peculiar

† = $\frac{1}{562}$ of an English inch.

‡ Professor R. Wagner finds a *single* spot in cartilaginous, and *many* spots in osseous Fishes. He finds also a *single* spot in the scaled, and *many* spots in the naked Amphibia.

§ Beiträge zur Geschichte der Zeugung und Entwickelung, tab. ii. fig. 6. *a.*, (aus den Abhandlungen der Mathematisch-physikalischen Klasse der Königl. Bayer. Akad. der Wissenschaften in München besonders abgedruckt).

|| "Chorion" of R. Wagner, from whom I borrow this figure. (Beiträge, &c., tab. ii. fig. 7 *c.*)

granules (8.) in the fluid of the ovisac. Compare with fig. 16. Plate V. from the Hog, and fig. 25. Plate V. from the common Fowl.

43. Plate VI. fig. 29. Ovisac of the common Frog; the yelk (*d*) becoming opake, and the germinal vesicle (*c*) in its centre.

44. Plate VI. fig. 36. Ovisac of the Salmon, acquiring a proper covering or tunic. In this instance two minute ovisacs are *included* (57.) by that portion of the covering which enters into the formation of the pedicle.

45. Plate VI. fig. 37. Ovisacs of the Salmon, no longer to be distinguished from their proper coverings or tunics, with which they have coalesced to form vesicles (capsules) analogous to the Graafian vesicle of Mammals. The vesicles have been crushed, and the yelk is seen escaping by the pedicles, taking with it its proper membrane (*e*). See the ideal section, Plate VI. fig. 38½; which shows why the yelk escapes by this passage.

46. Plate VI. fig. 38. Perfect calyx of the Salmon, with its pedicle.

The Graafian Vesicle of Mammals, how related to the Calyx of Birds, Amphibia, and Fishes.

47. Should the accuracy of the figures now compared be confirmed by future observation, it may perhaps be considered as established, in the first place, that the primitive elements of the ovum of Birds, Amphibia, and Fishes, are contained in a vesicle (the "chorion" of authors) essentially the same as that which I have called the *ovisac* of the Mammalia; secondly, that if it be, as I suppose, by acquiring a proper covering or tunic† that the ovisac of the Mammalia becomes what has been by others called a *Graafian vesicle*, the ovisac of Birds, &c. by acquiring a proper covering or tunic† presents a structure (capsule) analogous thereto; and thirdly, that it is the structure so constituted in the Bird, &c., which on becoming pendent from the ovary, and invested by what there is of the substance of the ovary, as well as in some instances by the peritoneum‡, has been called a calyx§.

48. From the foregoing it appears that the Graafian vesicle is not, as it has been supposed to be, a structure peculiar to Mammalia. Nor is it correct to consider the Graafian vesicle of Mammalia as analogous to the whole calyx of other animals. It corresponds to no more than the two internal membranes, viz. the ovisac and its proper vascular covering, the remainder of the calyx being derived from the ovary, with, in Birds, some Amphibia and some Fishes, a peritoneal covering; and however incon-

† Susceptible, as already said, of becoming highly vascular.

‡ In other instances the peritoneal investment does not enter into the formation of the calyx. We find examples in the naked Amphibia, in Ophidian and Saurian Reptiles, and in most osseous Fishes, where the ovaries are sacs, having the calyces pendent in their interior.

§ My researches have not yet been extended to the Invertebrata; but I am by no means disposed to consider the existence of the vesicle, which I have called the ovisac, as limited to vertebrated animals.

siderable this portion of the calyx may appear, yet for the sake of analogy the distinction is not unworthy of being made†.

The proper Membrane of the Yelk in Mammals.

49. Several distinguished authors have not described this membrane‡. And in fact it is not easy to demonstrate the separate existence of such a membrane in the class Mammalia generally, at those periods in the formation of the ovum when the latter is usually examined. On the other hand, a very accurate observer, KRAUSE, has figured it in the Goat§, and appears to have often seen it in other Mammals. My friend Professor RUDOLPH WAGNER pointed it out to me in the Cat; and he seems convinced from analogy that its existence must be general in the class Mammalia, but does not appear to have discerned it in any other instance||. I have since met with and figured it in early stages of the ovum of the same animal (the Cat), and I believe also in the Hog (Plate V. figs. 17. and 16. *e.*; Plate VIII. fig. 70. *e.*). In later stages, *ante coitum*, I have sought this membrane in vain as a distinct structure¶; but I think it not improbable that the well-marked line generally regarded as representing the internal surface of the thick chorion, may be partly due to the proper membrane of the yelk, as is obviously the case in Plate V. fig. 17. *e.* and *f.*

50. It appears to me that the existence of a proper membrane of the yelk in the ovary throughout the class Mammalia, may be inferred from what we know of the effect observed to be produced on ova by maceration; when the thick chorion, imbibing fluid into its interior, becomes distended beyond the size of the yelk-ball, but the latter *retains its form*, which certainly would not be the case were not the yelk circumscribed by a proper membrane (Plate VIII. fig. 72. *f.* and *d.*††).

51. I have observed a very interesting fact connected with the proper membrane of the yelk *post coitum* not hitherto made known, which not only adds probability to the supposition that this membrane has a previous existence, but also accords in some degree with a change known to take place in Birds. I find that in the Rabbit,

† Professor R. WAGNER in his examination of ova of the Frog, finding his "chorion" (ovisac) to disappear, makes the following inquiry: "Ist sie nun mit der Dotterhaut verschmolzen oder hat sie, was wahrscheinlicher ist, sich an die gefässreiche Kelch-membran angelegt und bildet sie nur die innere Lamelle derselben?" (Beiträge, &c., S. 76.) This inquiry, made by one of the most accurate of observers, bespeaks, I think, the perception, in a single instance, of something that was very near the truth.

‡ Among these are COSTE (Embryogénie Comparée, p. 80.) and T. W. JONES (London and Edinburgh Phil. Mag., No. 39, Sept. 1835, p. 210.).

§ MÜLLER's Archiv, 1837, Heft I. Taf. I. figs. 4, 5, 6.

|| Beiträge, &c., S. 20.

¶ Since the above was written, an examination of the ovum of the Goat enables me to attest the accuracy of Professor KRAUSE in his representation of the membrana vitelli in this animal, in which it is exceedingly distinct. I did not however, in any instance find the membrana vitelli surrounded by a fluid as described by KRAUSE (53. Note), but by the perfectly formed and consistent chorion.

†† I borrow this figure from the excellent Thesis of BERNHARDT (Symbolæ, &c., fig. xxiii. p. 45.), but am compelled to offer an explanation of several of its parts different from that given by the author. This is deferred, however, until after the description of the chorion (56. Note). See also the Description of the Plates.

just before the ovum leaves the ovary, this membrane, previously so delicately thin, becomes perfectly distinct and very thick; and that the chorion, imbibing fluid into its interior, becomes somewhat distended, so that a minute space is visible between the membrana vitelli and the chorion. This thickening of the proper membrane of the yelk, and the distention of the chorion, subsequently proceed much farther, as is proved by the state of ova found in the Fallopian tube. I find also, that the membrana vitelli is still visible, and has considerable thickness in minute ova met with in the uterus. This subject will be entered into more fully in a future paper.

The true Chorion, a Structure superadded within the Ovary in the Class Mammalia.

52. The figures above referred to, in comparing the *ovisac* of Mammalia with that of Birds, Amphibia, and Fishes, present also up to a certain period a perfect analogy between the rudiments of the *ovum* itself in these four classes. We have seen in all, the germinal vesicle and its contents, as well as the yelk, and proper membrane of the yelk. Here, however, the analogy is terminated by the formation, within the ovary, in Mammalia, of a membrane to which there is no corresponding structure *within the ovary* in other Vertebrata. This membrane appears to be the true *chorion*. In the ovary of Birds, Amphibia, and Fishes, it is, I believe, allowed that there is no membrane formed external to the membrana vitelli†. The body therefore expelled from the ovary in these animals is not an ovum, but a *yelk-ball*. The subject will be illustrated by the following Table, showing the parts present (in a mature state) in the ovary of Mammalia on the one hand, and of Birds, &c. on the other:—

Mammalia.	Birds, some Amphibia‡, and some Fishes‡.
Germinal vesicle (*c*) and its contents.	Germinal vesicle (*c*) and its contents.
Yelk, *d*.	Yelk, *d*.
Membrana vitelli, *e*.	Membrana vitelli, *e*.
Chorion, *f*.	
§ *Tunica granulosa*, g^1.	
§ *Retinacula*, g^2.	
Fluid, granules (*g*), and oil-like globules (*).	
§ Membrana granulosa, g^3.	
Ovisac, h. *Vascular tunic, i.* } = Graafian vesicle, *h i*.	*Ovisac, h,* *Vascular tunic, i.* } = Capsule, *h i* } = Calyx, *h i k l*.
Stroma, *k*.	Stroma, *k*.
Peritoneal covering, *l*.	Peritoneal covering, *l*.

† Professor RATHKE, however, finds that in certain Fishes, not provided with an oviduct, the "schaalenhaut" is a production of the ovary. (BURDACH's Physiologie, 1837, Band II. § 339.)

‡ In other Amphibia and most of the osseous Fishes, the peritoneum does not enter into the formation of the calyx (47. Note).

§ To be described in Part II. of this Memoir (64. 80. 72.).

The foregoing Table shows that in the ovary of Mammalia, the chorion is superadded; and as I shall demonstrate in a future communication (what others, and particularly COSTE and R. WAGNER, have conjectured) that this membrane is really the chorion of ova met with in the uterus†, it may be said that not merely a yelk-ball, but an *ovum* is expelled from the ovary of Mammals‡.

53. The chorion, as it exists in the ovary, is soft, very thick, and remarkably transparent. It closely invests the membrana vitelli, so as in nearly all instances to conceal the separate existence of this very delicate membrane (49.). When first formed, the inner portion of the substance of the chorion is probably in a fluid state, as is evident from Plate VIII. fig. 70. *f*., in which instance slight pressure at one side forced a part of the substance of this membrane to the side opposite§. (In this figure the inner circle (*e*) is probably the proper membrane of the yelk (49.).) Subsequently the chorion acquires more consistence; for if it be ruptured no collapse takes place even after the escape of its contents, and the membrane continues throughout of equal and undiminished thickness‖. I find, as already said (51.), that just before the ovum leaves the ovary this membrane begins to be attenuated by imbibition of fluid into its interior, and consequent distention. This imbibition of fluid and distention may be produced by maceration (Plate VIII. fig. 72.).

54. The great thickness and transparency of the chorion, as it exists in the ovary, long prevented its real nature from being ascertained, and appear to have been the means of misleading several excellent observers.

55. VON BAER, for instance, after describing the yelk as "une sphère creuse à paroi épaisse," says it is separated "par un *intervalle transparent* d'une autre sphère creuse à paroi mince¶." The "intervalle transparent" of Professor BAER is obviously the transparent substance of the chorion, and his "paroi mince" the external surface of this thick membrane. It appears to have been this supposed "paroi mince" that BAER considered as subsequently forming the "membrane corticale" of ova in the uterus.

† The membrane here denominated the true chorion is considered by COSTE as the "membrane vitelline" (Embryogénie Comparée, p. 80.). This author, however, as above said, is correct in supposing this membrane to become the chorion in the uterus.

‡ On the other hand, there is a structure formed in the ovary of Birds, and some other Vertebrata, which (though it has been described) I do not find existing *ante coitum, in the ovary* of Mammals. This is the incipient "germinal membrane" ("blastoderma"), to be considered in a future paper.

§ Professor KRAUSE supposes the membrana vitelli to be surrounded by a pellucid fluid, which is circumscribed by an extremely delicate membrane (MULLER's Archiv, 1837, Heft I. S. 27, 28; see also the description of these parts in KRAUSE's admirable work, "Handbuch der menschlichen Anatomie, Band i. S. 557, 558."). The Professor's description I find very accurate, as applied to the chorion in an early state; such, for instance, as that which I have figured in Plate VIII. fig. 70, above referred to; or possibly it may be applicable to the permanent condition of this membrane in certain animals. I have never met with this state, however, except in ova newly formed.

‖ BERNHARDT has thus demonstrated the nature of this membrane (Symbolæ, &c. fig. xx.); and a figure by R. WAGNER (Beiträge, &c., Tab. i. fig. 2.) shows it in a manner still more satisfactory.

¶ Commentaire, pp. 39, 40.

56. Bernhardt, as already said, by rupturing this membrane demonstrated its real nature. In cases, too, where he viewed the "ovulum" (ovum) freed from its surrounding granules, he appears to have regarded it, in its whole thickness, as the "membrana externa ovuli[†];" but in those instances in which granules were adherent to the ovum, concealing the outer surface of this thick membrane, the substance of the latter was mistaken by Bernhardt for a "zona pellucida," or "spatium pellucidum;" and the part which this author indicates as the "membrana ovuli externa," is obviously the internal surface of the chorion. See Plate VIII. fig. 72.[‡], and the description of the Plates.

Parasitic Ovisacs.

57. Minute ovisacs (*h*) are seen in Plate VIII. fig. 75, in the parietes of a Graafian vesicle (*h i*) of one of the Mammalia, the Dog. The same Plate, fig. 76, presents a number of ovisacs similarly situated in the corresponding structure of a bird, the Pigeon[§]. If such a Graafian vesicle or corresponding structure be crushed, the ovum contained in its cavity escapes, but the minute ovisacs in its parietes remain unmoved. I have counted more than fifty of the latter, scarcely exceeding their primitive size, in the parietes of one of these structures from the Bird, none of which probably would have been matured. Sometimes these minute ovisacs appear to have *originated* in the substance of the covering acquired by a larger ovisac; and sometimes they seem to be *included* by the covering which the larger ovisac acquires. An instance of the latter is seen in Plate VI. fig. 36, where two small ovisacs are so included[||]. The minute ovisacs in such a situation, whether originating there or contained in it by inclusion, I propose to call *parasitic ovisacs*.

58. Plate VIII. fig. 77. presents one of these parasitic ovisacs[¶] (*h*) contained in the parietes of a Graafian vesicle of the Ox. Both have been divided, one half of each remaining in the ovary, the other being reflected. At fig. 78. is an enlarged view of the two halves of the divided parasitic ovisac[¶] seen in fig. 77. The proper membrane (*h*) is seen lined by the membrana granulosa (g^3); and within the latter is a dark space, the cavity of the parasitic ovisac which contains the ovum (*f*). Fig. 79. presents a transverse section of this parasitic ovisac; *h* being its proper membrane, and *g* granules of the membrana granulosa. The proper membrane (*h*) of the parasitic ovisac in this instance measured $\frac{1}{5}'''$ in thickness, and its cavity distended under the compressor $1\frac{1}{2}'''$ in length.

† Symbolæ, &c., figs. ii. iii. iv. x. &c.

‡ Fig. xxiii. of Bernhardt. This is the figure before referred to in the description of the proper membrane of the yelk (50. Note).

§ I have seen minute ovisacs similarly situated in a Chelonian reptile.

|| In this instance the included ovisacs are contained in the incipient *pedicle* already mentioned (44).

¶ Incipient Graafian vesicle?

Isolated Spots in the Graafian Vesicle.

59. These *appear* to be of two kinds, but are really the same,—in the one case seen single, in the other grouped. Plate VIII. fig. 66. g^*. presents the appearance of these objects in the one state, and Plate V. fig. $26\frac{1}{2}$. their appearance in the other. Each of these spots I find to consist of one of the peculiar granules of the Graafian vesicle, having a peripheral accumulation consisting of oil-like globules†. The peculiar granules are transparent and comparatively large; the oil-like globules appear dark in their circumference by reason of a very different refracting power from that of the suspending fluid; they are also of a minuteness quite immeasurable. When these objects are in small number, they present the appearance shown in Plate VIII. fig. 66. g^*.; and when in very large number, that presented in Plate V. fig. $26\frac{1}{2}$.‡.

Disappearance of Ova, and Formation of others.

60. I have very often met with ova in the state represented in Plate VIII. fig. 67, which was observed in a Rabbit of about ten weeks old. The chorion (*f*) has become thin, distended, and of an elliptic form. It measured in this instance one-fifth of a Paris line in length§. The yelk (*d*) is nearly black, consisting chiefly of granules of extreme minuteness, with some oil-like globules and a fluid. Its membrane, if still existing, is not distinguishable from the chorion; and indeed the chorion itself is scarcely to be distinguished from the yelk. The germinal vesicle (*c*) is generally situated at one end of the ovum, as in this instance,—appears more or less flattened or collapsed,—and is much enlarged. It measured in this case $\frac{1}{15}$th of a line in length. The germinal spot (*b*) also was enlarged, having measured in this instance $\frac{1}{45}$th of a line in length, for it was somewhat elliptical. Its contour is not well marked. The colour of the Graafian vesicle, with such a condition of its ovum, is often tinged with yellow. In young Rabbits this state of the ovum has presented itself so frequently, that I have observed several in the same field of view. I apprehend it to denote a stage in the absorption of ova; which on the other hand appear to be continually replaced by new formations‖.

† Among the oil-like globules there are sometimes visible a few minute opake granules.

‡ Spots which I suppose to represent the first of the two states now described, have been figured in BERNHARDT's Dissertation, Symbolæ, &c., fig. xvii.; but the author's description of them does not quite accord with that which I have given.

§ $= \frac{1}{56}$ of an English inch.

‖ In a future paper it will be shown that, *post coitum*, many ova are absorbed. In Plate VI. figs. 44. and 46. are seen two ova in which absorption is incipient, as indicated by a loosening of the granules, and consequent enlargement of the tunica granulosa (g^1),—by the more globular form of some of these granules (9.),—and by liquefaction of the yelk around the germinal vesicle.

PART II.

On a granulous Tunic of the Ovum of Mammalia not hitherto described; on the manner of origin of the "Membrana Granulosa" of Authors; on the different situations of the Ovum in the Graafian Vesicle at certain periods ante coitum, not hitherto observed; and on certain structures by means of which the Ovum is made to occupy those several situations.

61. Professor BAER has described and figured the "ovulum" (ovum) of Mammalia as situated in a mass of granules at or near to the periphery of the vesicle of GRAAF (Plate VII. fig. 62.), and the following is the substance of the description he has given.

The "Disc" of Professor BAER.

62. This accumulation of granules, according to BAER, is not globular but discoid†, thicker at the central part‡, and surrounding the "ovulum" like the frame of a lens its glass, but probably extended so as completely to inclose the under surface of the "ovulum"§ in addition to its sides. The lateral portion of this mass of granules VON BAER calls the disc ("*discus proligerus*")‖, and the central thickened part he calls the "*cumulus strati proligeri*‖." This description is intended to apply to this part in the Mammalia generally; but VON BAER describes the form of the "cumulus" as varying in different animals, and in the same animal at different periods, perhaps according to its degree of maturity. Thus it is very inconsiderable in the Dog, requiring the greatest attention to discern that the disc is thickened at its middle: and here VON BAER compares the granulous accumulation to a plate used at table, as seen on its lower surface. In the Cow he has seen the "cumulus" cylindrical or globular. This convex middle part is also very considerable in other animals; in the human female it has the appearance of a depressed hemisphere; and in the Hog it presented very varied forms¶, the cause of which differences BAER was not able to explain††. In the Cow he saw a plane granulous substance, very distinct from the eminence ("cumulus"), extending itself at the surface of the fluid of the Graafian vesicle; but whether this was situated internal to the cumulus, or formed a part of it destined to be detached, he considered extremely difficult to decide‡‡. He adds, "when we examine an ovulum of the Sow or Cow under the microscope, we there see commonly adherent a portion more or less considerable of a flat disc. In the Sow I have seen this little membrane bounded by a circular border; I believe that I have sometimes obtained it entire. In the Dog the disc is always limited by a circular border, but in this animal the membrana granulosa is too small to admit of being well examined. In the ovary of the human female I think I have distinctly

† Lettre, &c., p. 15. ‡ *L. c.*, p. 16. § *L. c.*, p. 16.

‖ He gives it this name from the supposition of its analogy with the granules surrounding the germinal vesicle of Birds.

¶ *L. c.*, p. 16. †† Commentaire, &c., p. 40. ‡‡ Commentaire, &c., p. 40.

seen the membrana granulosa to pass above the discus proligerus. On the immature ovulum [ovum] of the Cow, on the contrary, I have recognised no more than a continuous lamina, which I consider as the membrana granulosa. From all that has been said, I think I may conclude that the ovula [ova] of the Mammalia, those at least which are approaching maturity, are plunged into a couch, formed of a thick eminence, the cumulus, and of a flat disc, and that the latter may or may not be primitively isolated from the membrana granulosa; development predominating sometimes on the part of the disc, sometimes on the part of the cumulus, examples of which are offered (respectively) by the Bitch and Cow†." See Plate VII. fig. 62. and Plate VIII. fig. 80.‡.

63. This description appears to be accurate in a few particulars, but for the most part it does not accord with my observations. I have naturally felt some hesitation in stating this, not only from its being opposed to the opinion of so experienced and, generally speaking, so accurate an observer, but because those who have succeeded Baer in these researches appear to have adopted entirely his description of this structure. In now offering a different account of it, I would express my belief that should this prove more accurate, it will have been rather due to my examination of the ovum when situated in the *centre* (as well as near the surface) of the Graafian vesicle, than to imperfect observation on the part of those eminent observers. But I must first describe a structure which, to my surprise, has not been hitherto observed, though visible in those situations in which the ovum has been usually examined.

The Tunica granulosa.

64. On the rupture of a Graafian vesicle under the compressor, the ovum escapes, and with it a mass of the peculiar granules I have at some length described (8.). This mass of granules, as hitherto obtained, has presented the appearance of a lacerated structure, and been regarded simply as a portion of the "disc" of Baer§. I find, however, that if the mass thus escaping be attentively examined, those granules that immediately surround the ovum appear to be in a state of denser aggregation than the rest, from which they are to be distinguished therefore by a circumscribing line. If the granules thus circumscribed be viewed at different distances, they are seen to invest the whole surface of the ovum, forming a tunic, which is perfectly spherical in form (Plate VI. figs. 42. to 47. g^1.). I have frequently obtained the ovum invested by no other granules than those of this tunic; that is, this tunic has been freed from the other granules, which on the bursting of a Graafian vesicle generally escape adherent to it; the fact being, that all that properly belongs to the ovum of the mass of granules in which it is found imbedded in the Graafian vesicle is this

† Commentaire, &c., p. 40.

‡ Lettre, &c., figs. ix. and xii. of Baer. These figures I have introduced for the purpose of contrasting them with my own drawings.

§ This lacerated structure is the "zona granulosa" of Bernhardt, Symbolæ, &c., figs. i. vii. xviii. xxi. xxiii. See also (in the present memoir) Plate VIII. fig. 72. g^1., which is taken from that author.

tunic, between which and the surrounding mass there is much less adhesion than there is between the granules of either among themselves. Having never failed to find this tunic enveloping mature ova in the Mammalia, I believe its presence to be constant and essential; and I am also of opinion that in form, substance, and situation it is essentially the same throughout this class of animals. I propose to call it the granulous tunic of the ovum, *tunica granulosa*.

65. At a certain period this tunic, in some animals at least, is seen to have tail-like appendages consisting of granules precisely similar to its own (Plate VII. fig. 63. g^1.). These appendages appear to be frequently four in number, and their direction corresponds to that of other granulous cords or bands to be presently described (86.). They are not very distinctly seen while within the Graafian vesicle, and in this situation I have not figured them.

66. Should the existence of this structure as a *tunic* be confirmed by the observations of others, it will no longer be surprising that the outer line in the double contour of the thick chorion remained so long unseen, and that this thick membrane was regarded as a "zone," "halo," or "pellucid space." For the same reason it will be obvious why it has been found so difficult to free the ovum from the surrounding mass of granules; though I find that these granules do not adhere to the chorion they invest, so closely as to each other.

67. The tunica granulosa may be obtained in an uninjured state, either by carefully opening a Graafian vesicle of considerable size, and receiving its fluid and granulous contents into a watch-glass (Plate VII. fig. 61. g^1.), or by bursting one of middling size under the microscope with the compressor, when the escape of the ovum in its tunica granulosa may be observed. In both cases, however, portions of another granulous structure (the retinacula, to be presently described (80.),) generally escape adherent to it. This tunic is also very distinctly visible *in situ*, as in Plate VI. figs. 39, 40. and Plate VII. figs. 55. to 59. g^1.

68. I have met with the ovum when immature, and apparently just after the formation of the chorion (Plate V. fig. 17. *f*.), at which period the tunica granulosa does not exist; and in subsequent stages its gradual formation may be observed, as in Plate VI. fig. 41. g^1., where its thickness is inconsiderable and its contour irregular.

69. The tunica granulosa, as already said, is spherical. In whatever direction it is viewed, whether as in Plate VII. fig. 61. g^1., or in any other of the figures of this Plate, there is perceptible no difference in its form; the distinctness with which the contained ovum is seen, depending, not on the absence of the investing granules at any part of its periphery, but on the extreme transparence of these granules. The flattening at one side which this tunic may undergo, and the entire deprivation of granules, perhaps, at one point of the surface of the chorion, by pressure against the membrane of the ovisac, I shall have to notice (88.) †.

† I shall presently describe a structure supposed, as already said (56.), by Professor Baer to be "discoid" in its form. In the *Dog*, however, this author has obviously figured as the "*discus proligerus*" the structure

70. The granules forming this tunic being soluble in water (9.), the fluid of a large Graafian vesicle, or some thin albumen, is a preferable medium in which to view this structure, as well as all the other parts connected with the ovum.

71. In a future paper I shall show that the tunica granulosa accompanies the ovum into the Fallopian tube.

The Membrana granulosa.

72. Von Baer† gave this name to a stratum of granules lining the internal surface of the Graafian vesicle. That author's description of the membrana granulosa appears to be very accurate so far as it goes; but there remains something to be said respecting it, for to this day doubts are entertained whether this stratum of granules really constitutes a membrane, though Dr. Pockels recently demonstrated it to be such in a mature Graafian vesicle of the Deer‡. Von Baer has also figured it§, but so little magnified that no adequate idea is thus given of this membrane.

73. It is composed of the peculiar granules of the ovisac (Plate VIII. fig. 71.). With the proper membrane of the ovisac, now the lining membrane of the Graafian vesicle, the membrana granulosa is in contact only||, appearing very easily to break down and separate from it. The membrana granulosa may be made to escape by rupturing a Graafian vesicle under the compressor; when, after the escape of the other contents, it slowly passes out in a membranous form¶. It is then possible to roll portions of this membrane on themselves, and even to obtain folds, as seen in Plate VIII. fig. 71.

74. The granules forming the internal surface of this membrane are sometimes found less densely aggregated together than those in other parts; portions of the membrane projecting like villi into the fluid of the Graafian vesicle. This membrane is also frequently found to be of very unequal thickness, projecting at some parts much farther into the cavity of the Graafian vesicle than at others. The cause of these apparent villosities and projections will be presently explained, when the manner of origin also of this membrane will be shown (79.).

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which I am now describing as the *spherical* tunica granulosa. He probably saw it in no other state than that to which I have just referred, viz. flattened or removed by pressure against the membrane of the ovisac.

† Lettre, &c., p. 18, and Commentaire, p. 47.

‡ Müller's Archiv, 1836, Heft ii. S. 193—204. Tab. vi.

§ Lettre, &c., fig. xi.

|| It was observed by Baer, that between the membrana granulosa and the internal surface of the Graafian vesicle there was no "liaison organique." Commentaire, p. 41.

By maceration, as observed by Baer also, the membranous structure of this granulous lining may be well seen *in situ* from the exterior of a Graafian vesicle; the membrana granulosa separating as a distinct vesicle, and a space intervening between it and the walls of the cavity it lines.

¶ It is often more yellow in its colour than the other granulous contents of the Graafian vesicle.

The Ovum in the centre of the Ovisac. Manner of origin of the Membrana granulosa.

75. The formation of the ovum, shown in Part I. of this memoir (12—22.) to commence before the existence of the ovisac, is completed in the number of its parts, (though probably not matured) in or near the *centre* of this vesicle (Plate V. fig. 17.).

76. But if the ovum at this period is situated in the centre of the ovisac, by what means is it supported there? At first this appears to be effected by an equable diffusion of the peculiar granules of the ovisac throughout the fluid of this vesicle (Plate V. fig. 17.). Subsequently a peculiar structure arises partly for this purpose.

77. The ovisac, as already said (24. 25.), acquires a proper covering or tunic susceptible of becoming highly vascular, and thus passes into the condition of what has been denominated a Graafian vesicle. As this covering or tunic begins to form at the period just referred to, viz. while the ovum is still in the *centre* of its containing cavity, it will be proper in future to lay aside the term *ovisac* and to substitute that of *Graafian vesicle,* the former being now no more than the lining membrane of the latter.

78. The granulous contents of the *Graafian vesicle* now assume an amygdaloidal appearance (Plate VII. fig. 48.), which seems to arise from a denser aggregation of the granules in certain parts, and the occupation, by the fluid, of the spaces thus occasioned; in other words from a *histological* separation. If examined more closely, however, the condition of the interior of the ovisac is not found to be really amygdaloidal, as the cavities communicate with one another (Plate VII. fig. 49.).

79. In a stage somewhat more advanced this separation is found to have proceeded farther, the peculiar granules of the Graafian vesicle having arranged themselves so as to constitute three distinct structures (Plate VII. fig. 50. g^1, g^2, g^3.). Some of them are collected on the surface of the chorion, forming the *tunica granulosa* (g^1), which I have already described (64.); others constitute a structure, or rather an assemblage of structures, consisting of a central mass, in which the ovum (in its granulous tunic) is contained, and of cords extending from the membrana granulosa to this central mass (g^2), which will be now described; and the third portion of these granules is collected on the inner surface of the Graafian vesicle, where they constitute the membrana granulosa (g^3), *this being, I apprehend, the manner of origin of this membrane.* See the sixth column in the table, page 304.

The Retinacula.

80. The central mass and cords of granules just mentioned (Plate VII. fig. 50. g^2.) do not appear to have been hitherto observed. One of their uses will perhaps be apparent from the figure now referred to. They obviously suspend the ovum, and retain it in its situation in the fluid of the Graafian vesicle; from which office, with others to be hereafter mentioned, I propose to name these structures the *Retinacula*†.

† The granulous cords extending from the membrana granulosa to the central mass are probably much more numerous at this period than represented in the figure; but the minuter ones could not in this instance be seen, nor are the minutest ever visible, from the exterior of the Graafian vesicle.

81. Every observer who has burst a Graafian vesicle under the microscope must have noticed the mass of granules that always escapes with the ovum. Von Baer, indeed, mentions "shreds" formed of granules as being visible in the mass thus escaping. The structures I have just mentioned (*retinacula*) (Plate VII. fig. 50. g^2.) explain the nature of the escaping mass of granules, and the "shreds" observed by Baer, which latter appear to have been the ruptured retinacula.

82. When describing the membrana granulosa (74.), I stated it to be sometimes of very unequal thickness, and to present villi projecting from its inner surface into the fluid of the Graafian vesicle. If Plate VII. fig. 50. g^2 and g^3 be again referred to, and the manner of origin, as I suppose, of the membrana granulosa borne in mind, the cause of those inequalities in the thickness of the latter will be evident. The villi on the internal surface of this membrane are nothing more than the remains of the retinacula, broken off as these are when the contents of a Graafian vesicle escape†.

83. The retinacula as now described, extending from the membrana granulosa to the centre of the Graafian vesicle, are in most animals very indistinctly seen, and hence it is that they have been hitherto overlooked. I first saw them in the Rabbit, and to those who may wish to convince themselves of their existence I recommend this animal, from the comparative distinctness with which they are here seen (Plate VII. fig. 50. g^2.). They are very distinct also in the Ferret (*Mustela Furo*); less so, yet visible, from the exterior of the ovisac in the Ruminantia (figs. 51 and 52. g^2.): and having never failed to find some trace of them, it is my opinion that these structures are general in the class Mammalia‡.

84. Very often when the central mass is visible, and the ovum distinct within it, nothing is seen of the cord-like portions of the retinacula. This is sometimes owing to the size of the Graafian vesicle rendering the object too remote, or to the imperfect transparency of its membranes, and sometimes to undue pressure having been applied, which breaks the cord-like portions down. Care must be taken, when they are sought for, to apply no more pressure than is absolutely needful; and perhaps the general remark may not here be out of place, that even with the most careful manipulation these parts may sometimes become displaced. Dissecting out a Graafian vesicle, stripping off the peritoneal covering of the ovary, or slicing and even handling the latter, tend to disturb the exquisitely suspended ovum.

The Ovum conveyed to the periphery of the Graafian Vesicle.

85. From the centre of the Graafian vesicle the ovum passes to its periphery. This change of place is gradual, and the same structures which we have seen to suspend the ovum in the former situation convey it to the latter. In Plate VII. fig. 55. this

† Although for the sake of perspicuity I have thought it best to distinguish the membrana granulosa and retinacula by two appellations, I think they are but parts of the same structure.

‡ I have seen these structures (as well as the tunica granulosa) in the Goat, the Hedgehog, and the Human subject.

change is seen in profile taking place. The granulous cords that are situated on one side of the central mass have in this instance disappeared, while those on the other side have become shortened, until the ovum has nearly reached the periphery of its Graafian vesicle. And what is very remarkable, and an interesting instance of *design,* the particular part of the periphery of the Graafian vesicle to which the ovum is thus conveyed, is always that directed towards the surface of the ovary.

The Ovum at the periphery of the Graafian Vesicle†.

86. Nor does the office of the retinacula end here. Having suspended the ovum in the centre, and conveyed it to the periphery of the Graafian vesicle, they now retain it in the latter situation (Plate VII. figs. 55 to 59. g^2.‡). The central mass in which the ovum is contained has become smaller, in many instances nearly of the size and form of the tunica granulosa (g^1), and in some animals appears more or less distinctly circumscribed by a membrane§, a continuation of which invests the cord-like, or at this period the band-like portions of the retinacula‖. The band-like portions of the retinacula have become reduced frequently to four in number, which are at right angles to each other (figs. 58 and 59. g^2.). Sometimes only two remain, and I have met with more than four (figs. 56 and 57. g^2.). The retinacula often seem closely pressed against the inner membrane of the Graafian vesicle, for which purpose, apparently, a portion of the membrana granulosa is in some instances previously removed (figs. 57 and 59. g^3.). In others the membrana granulosa is quite entire.

87. In Plate VII. fig. 61. is seen a fragment (g^2) of the central portion of the retinacula, from which the granulous bands have been broken off¶; and fig. 60. presents the same part *in situ,* and of the real size, as viewed in a large Graafian vesicle (*h i*) removed from the ovary, laid open, and its outer third reflected.

88. In the determinate pressure of the ovum towards the periphery of its Graafian vesicle, the retinacula obviously perform an important part††. One effect of this de-

† See the seventh column in the Table, page 304.

‡ One of the figures now referred to (fig. 58. from the Ferret,) presents a curious resemblance to the eye-ball with nearly all its parts, including the four *recti* muscles.

§ It is generally supposed that the surface of the "ovulum" (ovum) is in contact with the wall of its containing cavity; which however cannot be the case if the membrane now mentioned circumscribe the central portion of the retinacula on all sides.

In some animals that portion of this membrane which invests the central part of the retinacula is exceedingly distinct, as for instance in the Cat and Dog.

This membrane sometimes presents wrinkles or folds, as if susceptible of enlargement.

‖ It is not improbable that a continuation of this membrane, reflected from the retinacula, may invest the internal surface of the membrana granulosa (82. Note).

¶ Drawn as seen lying in a watch-glass.

†† In the Dog the retinacula generally become at this period very obscure (Plate VIII. fig. 66. g^2.), often quite imperceptible; and their central portion is reduced to a distinct membrane, closely investing the tunica granulosa (fig. 66. g^1. and g^2.); this membrane being, I presume, the "circular border" mentioned by BAER as circumscribing his "disc" (tunica granulosa) in this animal. That the retinacula, however, though generally indistinct, and indeed invisible, in this animal, continue to exercise an influence on the situation of the ovum,

terminate pressure appears to be penetration through the membrana granulosa, when the latter has not been previously removed. Another effect, probably in all instances, is the attenuation or removal, at one point, of the proper granulous tunic of the ovum (69.). By continued pressure, also, the retinacula may contribute to attenuate that part of the inner membrane (formerly the ovisac) of the Graafian vesicle in contact with which they lie†, and thus promote the expulsion of the ovum from the ovary.

89. All those who have been engaged in researches of this kind, must have noticed that on the bursting of an ovisac under the compressor, the ovum and a mass of granules are among the parts escaping first. This has been mentioned as remarkable. I think it will appear less so if the situation of the ovum as seen in most of the figures in Plate VII. be considered.

90. The whitish or yellowish white speck visible with the naked eye from the exterior of the ovary in the Dog, which led to Baer's discovery of his "ovulum" (ovum) of Mammalia, appears to be in this animal the tunica granulosa rather than the ovum. In some others in which such specks are visible from the exterior of the ovary, the central portion of the retinacula is also seen. This I find to be the case in the Rabbit and the Ferret, and in minute Graafian vesicles of the Ox. In the Rabbit I have with a good pocket lens discerned, from the exterior of the ovary, several of the *band-like portions* of the retinacula; and this I find possible in minute Graafian vesicles even of the Ox.

91. I have now gone through a minute description of the parts concerned in producing those appearances denominated by Von Baer the "cumulus" and "disc," as figured in Plate VII. fig. 62., and Plate VIII. fig. 80. If these figures, taken from that author‡, be contrasted with g^2 of my own figures in Plate VII., and particularly of fig. 55., it will be obvious that the "cumulus" of Baer is made up of what I have called the tunica granulosa, and the central portion of the retinacula; and that the band-like portions collectively of what I have called the retinacula, mainly contribute to present the appearance denominated the "flat disc" by Baer.

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is rendered probable by the fact, that when gentle compression has in some degree displaced this vesicle, it resumes its previous situation as soon as the pressure has been removed. This fact was observed by Professor Baer, but from not having seen the true form of the structures here called the retinacula, he was not in a condition to explain it.

One of the figures now referred to, from the Dog (Plate VIII. fig. 66.), presents four ova in one Graafian vesicle, which number, as well as three, I have repeatedly met with in this animal. Professor Valentin has also figured three in one Graafian vesicle of the Dog (Dissertation by Bernhardt, "Symbolæ," &c., fig. viii.). Two ova are very frequently present in the same Graafian vesicle in this animal; but in the Ferret (*Mustela Furo*) this is still more frequently the case. In the last-mentioned animal indeed I saw in many instances three, and not rarely four, ova in one Graafian vesicle. When this is the case, it is not unusual to find one or two ova apparently defective. To explain the existence of a plurality of ova in one Graafian vesicle, we must suppose that in such instances the membrane of the ovisac forms around, and thus includes in one cavity, the granulous envelopes of a corresponding number of germinal vesicles (23.).

† Von Baer, without having seen the peculiar structure of the retinacula, conjectured that this effect might be produced by the "ovulum" (ovum) on the parietes of its Graafian vesicle.

‡ Figs. ix. and xii. of Baer, Lettre, &c.

The Tunica granulosa and Retinacula of Mammals compared with the Membrana chalazifera and Chalazæ of Birds.

92. After what has been stated on the subject of the tunica granulosa and retinacula, it may by some be supposed that these structures are analogous to the membrana chalazifera and chalazæ of the Bird. They differ however in several respects. Thus the tunica granulosa and retinacula invest the *chorion*, while the membrana chalazifera invests the *proper membrane of the yelk*. The former exist *ante* coitum, the latter are formed *post* coitum. The retinacula and chalazæ may agree in the function of both having reference to the *position* of the ovum, but the *object* of the positions they respectively influence is essentially different, the one determining the situations of the ovum in the ovary, and assisting in its expulsion from that organ; the other (as it is supposed) performing a function which has reference to the parent heat in incubation.

Recapitulation.

93. In Mammalia and in Birds the germinal vesicle and its contents are those parts of the ovum which are first formed. This is probably the case in ova throughout the animal kingdom (12—22.).

94. The germinal vesicle at an early period is surrounded, in Mammalia and in Birds, by oil-like globules and peculiar granules, forming an envelope not hitherto described (14.).

95. The ovum of all vertebrated animals is contained in a vesicle, (the "chorion" of some authors, as found in Birds, Amphibia, and Fishes,) which is essentially the same in structure wherever found, and which it is desirable universally to denominate an *ovisac* (2. 47.). This vesicle is the "couche *interne*" of the Graafian vesicle of Mammalia as described by Baer (24. 25.).

96. The Graafian vesicle of the Mammalia is an ovisac that has acquired a proper covering or tunic, susceptible of becoming highly vascular, which covering is the "couche *externe*" of the Graafian vesicle as described by Baer (25.).

97. The ovisac of Birds, Amphibia, and Fishes ("chorion" of some authors) acquires in like manner a proper covering or tunic, susceptible of becoming highly vascular; and by the union of the ovisac with this covering, there is constituted a structure analogous to the Graafian vesicle of Mammalia (47.). The quantity of yelk in the former being large, that portion of the ovary which contains the structure here referred to (as analogous to the Graafian vesicle of Mammals) becomes pendent; and now the united coverings of the yelk-ball, viz. the ovisac, its proper tunic, the ovarian substance, and in some instances the peritoneal investment, are together called the *calyx* (32. 45. 47.). From this it will be obvious that the Graafian vesicle is not, as it has been supposed to be, a structure peculiar to Mammalia; nor is it correct to consider the Graafian vesicle of Mammalia analogous to the whole calyx of other animals (48.).

98. The ovisac has at first an elliptical or ellipsoidal form, becomes more spherical, and in certain Mammals somewhat tapered at one end (2. 34. 3.).

99. The structure of the ovisac in some of the Mammalia may be examined when it does not exceed in length the 50th or even the 100th part of a Paris line, that is, in the latter case, the 1125th of an English inch (4.).

100. Myriads of ovisacs with their contents are formed that never reach maturity (2.).

101. Some of the ovisacs which do not reach maturity are situated in the parietes of Graafian vesicles in Mammalia, or of the corresponding structures in other Vertebrata; being sometimes formed in this situation, and sometimes included by the covering which the larger ovisac acquires. The minute ovisacs so situated it is proposed to denominate *parasitic* ovisacs (57.).

102. The ovisac is often found in a cavity proper to itself, with the walls of which it has no organic union (6.).

103. The granules forming the envelope of the germinal vesicle above referred to, and subsequently found in the fluid of the ovisac, are very peculiar in their appearance; they are generally of an elliptic form and flattened, highly transparent, contain a nucleus and sometimes a pellucid fluid also, and are intimately connected with the evolution of the ovum (8. 10.). These granules are present in largest quantity in the ovisac of Mammalia; yet granules essentially the same exist at an early period in the ovisac of Birds (32. 33.), and are sometimes met with in that of Fishes (34. 42.).

104. A continual disappearance of ova, and the formation of others, are observable even at a very early age (60.).

105. The ovum of Mammalia when completely formed is at first situated in or near the *centre* of the ovisac (75.).

106. It is at this period supported in the centre of the ovisac by an equable diffusion of granules throughout the fluid of the latter (76.).

107. The ovisac about the same time begins to acquire its proper covering or tunic, by which addition, as already stated, there is constituted a Graafian vesicle, and of the latter the ovisac is now the inner membrane. After this period, therefore, it is proper to speak, not of an ovisac, but of a Graafian vesicle (77.).

108. The peculiar granules of the Graafian vesicle arrange themselves to form three structures, viz. the *membrana granulosa* of authors, and two structures not hitherto described, one of which it is proposed to name the *tunica granulosa*, and the other, which is rather an assemblage of structures than a single structure, the *retinacula* (78. 79. 82—Note.).

109. The tunica granulosa is a spherical covering proper to the ovum, and explains why the outer line in the double contour of the thick chorion, that is, the external surface of this membrane, remained so long unobserved. At a certain period, this tunic, in some animals at least, is seen to have tail-like appendages consisting of granules similar to its own (64—71.).

110. The retinacula consist of a central mass containing the ovum in its tunica granulosa, and of cords or bands extending from this central mass to the membrana granulosa. These structures at a certain period become invested by a membrane (80—84.).

111. The office of the retinacula appears to be, first, to suspend the ovum in the fluid of the Graafian vesicle; next, to convey it to a certain part of the periphery of this vesicle; subsequently to retain it in the latter situation; and finally, to promote its expulsion from the ovary (80—88.).

112. The particular part of the periphery of the Graafian vesicle to which the ovum is conveyed, is uniformly that directed towards the surface of the ovary (85.).

113. The mass of granules escaping with the ovum on the bursting of a Graafian vesicle under the compressor, is composed chiefly of the tunica granulosa and the ruptured retinacula (64. 81.).

114. The "cumulus" of Professor Baer is made up of the parts here called the tunica granulosa and the central portion of the retinacula; and the band-like portions, collectively, of what are here called the retinacula, mainly contribute to produce the appearance denominated the "flat disc" by that author (91.).

115. In Mammals a thick and highly transparent membrane, the true chorion, is formed external to the proper membrane of the yelk, while the latter is in the ovary. The inner part of the substance of the chorion in its early stages is in a fluid state, but it subsequently acquires more consistence. There is not any corresponding structure *within the ovary* of other vertebrated animals; so that the body expelled from that organ in the latter, is not (as in Mammalia) a true ovum, but a yelk-ball (52—56.).

116. The following appears to be the order of formation, as to time, of the more permanent parts of the ovum and the Graafian vesicle, in Mammalia, viz.

1. The germinal vesicle, with its contents (12—22.).
2. An envelope consisting of oil-like globules and peculiar granules (14—16.).
3. The ovisac, which forms around this envelope (23.).
4. The yelk, which forms within the ovisac around the germinal vesicle (24.).
5. The proper membrane of the yelk, which makes its appearance while the yelk is still in an incipient state (24. 49.).
6. The chorion (24. 52.).
7. The proper covering or tunic of the ovisac (77.); and about the same time the peculiar granules of the ovisac arrange themselves (78. 79.) to form,
   - The tunica granulosa (64.),
   - The retinacula (80.), and
   - The membrana granulosa (72.).

117. Such of these structures as are present in other vertebrated animals originate in the same order as to time (27—47.).

118. *Table of Measurements.*

The measurements are given in fractions of a Paris line ($'''$), the micrometer used, one of FRAUENHOFER'S, being divided according to French measure. The French inch (of twelve lines) is to the English inch, as 1·06575 is to 1·00000, or nearly one fifteenth more. Assuming it to be exactly one fifteenth more, the simplest mode of converting the fraction of a French *line* into the fraction of an English *inch*, will be to multiply the denominator of the former by the number 11·25 (or $11\frac{1}{4}$). Thus, the actual length of the smallest ovisac in fig. 9. from the Dog, measuring $\frac{1}{100}$th of a Paris line, is found to be $\frac{1}{1125}$th of an English inch.

The compressor having been generally used in these researches, some allowance must be made on this account, the actual size of the objects *h* and *h i* being rather below the measurements given in the Table.

When the object is elliptical, it is the long diameter the measurement of which is given in the Table.

No. of Figure.	Animal.	Diameters magnified.	Actual Dimensions.					
			b. Germinal spot.	*c.* Germinal vesicle.	*f.* Chorion (ovum).	g^1. Tunica granulosa.	*h.* Ovisac.	*h i.* Graafian vesicle, or corresponding structure.
1.	Rabbit........	440	$\frac{1}{525}$	$\frac{1}{150}$ to $\frac{1}{25}$				
2†.	Cat	440						
3‡.	Cat	440						
4.	Ox (Heifer)	100	......	....	....	....	$\frac{1}{60}$ to $\frac{1}{25}$, majority about $\frac{1}{50}$.	
5.	Sheep	60	......	....	....	....	$\frac{1}{30}$ to $\frac{1}{20}$, $\frac{1}{5}$	
6.	Cat	100	......	....	....	....	$\frac{1}{50}$ and upwards.	
8.	Cat	440	......	....	....	....	$\frac{1}{50}$	
9.	Dog	440	......	....	....	....	$\frac{1}{100}$ to $\frac{1}{25}$	
10.	Cat	240	......	....	....	....	$\frac{1}{50}$ to $\frac{1}{17}$	
11§.	Man..........	440	......	....	....	....	$\frac{1}{25}$	
12.	Man..........	150	......	....	....	....	$\frac{1}{25}$ and upwards.	
13.	Cat	440	$\frac{1}{900}$	$\frac{1}{150}$	....	....	$\frac{1}{25}$	
14.	Hog	240	......	$\frac{1}{110}$	....	....	$\frac{1}{25}$	
15.	Hog	240	......	....	....	....	$\frac{1}{20}$	
16.	Hog	240	......	....	....	....	$\frac{1}{17}$	
17‖.	Cat	440	$\frac{1}{300}$	$\frac{1}{50}$	$\frac{1}{12}$	....	$\frac{1}{7}$	
22.	Pigeon	440	......	$\frac{1}{200}$ to $\frac{1}{175}$	....	....	$\frac{1}{33}$ to $\frac{1}{25}$	
23.	Common Fowl..	440 ?	......	$\frac{1}{100}$	....	....	$\frac{1}{70}$	

† The whole object $\frac{1}{75}'''$. ‡ The whole object $\frac{1}{50}'''$. § The peculiar granules $\frac{1}{100}'''$.

‖ Oil-like globules, some of them $\frac{1}{200}'''$ and upwards. The peculiar granules, mean length $\frac{1}{150}'''$.

Table of Measurements. (Continued.)

No. of Figure.	Animal.	Diameters magnified.	Actual Dimensions.					
			b. Germinal spot.	*c.* Germinal vesicle.	*f.* Chorion (ovum).	g^1. Tunica granulosa.	*h.* Ovisac.	*h i.* Graafian vesicle, or corresponding structure.
24.	Common Fowl..	440	......	....	....	....	$\frac{1}{17}$	
25.	Common Fowl..	440	......	....	....	....	$\frac{1}{14}$	
26.	Common Fowl..	100	......	....	....	....	....	$\frac{1}{5}$
27.	Frog..........	?	......	....	....	....	$\frac{1}{50}$	
28†.	Frog..........	100	......	$\frac{1}{14}$ to $\frac{1}{13}$	....	....	$\frac{1}{5}$	
29.	Frog..........	60	......	$\frac{1}{8}$	....	....	$\frac{2}{5}$	
30.	Salmon	240	......	$\frac{1}{50}$ and less				
31.	Cod	100	......	$\frac{1}{100}$ to $\frac{1}{25}$	....	....	$\frac{1}{40}$ to $\frac{1}{10}$	
32.	Whiting	100	......	$\frac{1}{25}$ to $\frac{1}{20}$	....	....	$\frac{1}{8}$ to $\frac{1}{5}$	
33.	Haddock	100	......	$\frac{1}{60}$	....	....	$\frac{1}{20}$	
34.	Turbot	240	......	$\frac{1}{20}$	....	....	$\frac{1}{10}$	
35.	Salmon	240	......	....	....	....	$\frac{1}{10}$ to $\frac{1}{8}$	
36.	Salmon	100?	......	....	....	....	....	$\frac{1}{3}$
37.	Salmon	25	......	....	....	....	....	$\frac{4}{5}$
39.	Dog	60	......	....	$\frac{1}{17}$	$\frac{1}{14}$	....	$\frac{1}{3}$
40.	Mole	100	......	....	$\frac{1}{17}$	$\frac{1}{14}$	....	$\frac{1}{5}$
41.	Mole	440	$\frac{1}{250}$	$\frac{1}{70}$	$\frac{1}{20}$	$\frac{1}{18}$	$\frac{1}{8}$	
42.	Mole	240	......	$\frac{1}{60}$	$\frac{1}{14}$	$\frac{1}{12}$		
43.	Sheep	240	$\frac{1}{150}$	$\frac{1}{40}$	$\frac{1}{14}$	$\frac{1}{10}$		
44.	Dog	440	......	....	$\frac{1}{12}$	$\frac{1}{9}$		
45.	Hog	150	......	....	$\frac{1}{16}$	$\frac{1}{13}$		
46.	Hog	100	......	....	$\frac{1}{16}$	$\frac{1}{10}$		
47.	Dog	150	......	....	$\frac{1}{14}$	$\frac{1}{11}$		
64.	Hog	100	......	....	....	....	....	$\frac{1}{4}$
65.	Mouse	100	......	....	$\frac{1}{30}$	....	....	$\frac{1}{10}$
66.	Dog	100	......	....	$\frac{1}{32}$ to $\frac{1}{16}$	....	....	$\frac{1}{3}$
67‡.	Rabbit	100	$\frac{1}{45}$	$\frac{1}{15}$	$\frac{1}{5}$	....	....	$\frac{1}{4}$
68.	Dog	?	......	....	....	....	$\frac{1}{30}$ to $\frac{1}{5}$	$\frac{1}{4}$ to $\frac{1}{3}$
69.	Sheep	60	$\frac{1}{250}$	$\frac{1}{50}$	$\frac{1}{20}$	....	$\frac{2}{5}$	
71§.	Hog	100						
74.	Hog	60	......	....	$\frac{1}{30}$ to $\frac{1}{20}$	....	$\frac{1}{4}$ to $1\frac{1}{4}$	
75.	Dog	100	......	....	....	....	$\frac{1}{30}$ to $\frac{1}{20}$	$\frac{1}{2}$
76.	Pigeon	60	......	....	....	....	$\frac{1}{30}$	$\frac{1}{3}$
79‖.	Ox (Heifer)....	25						

† The spots, marked d', $\frac{1}{25}'''$.

‡ The ovum in this instance was enlarged, and probably in the course of being absorbed.

§ g^3, Membrana granulosa $\frac{1}{75}'''$ in thickness.

‖ h, Proper membrane of the parasitic ovisac (incipient Graafian vesicle?) $\frac{1}{5}'''$ in thickness; its cavity $1\frac{1}{2}'''$ in length when crushed.

119. Explanation of the Plates.

N.B. In all the figures the same letters denote the same objects, the alphabetical order of these letters corresponding with the order of position of the parts in proceeding outwards from the centre of the ovum: viz.

a. Fluid, or granulous fluid, of the germinal vesicle.
b. Germinal spot or germinal spots.
c. Germinal vesicle.
d. Yelk.
  d'. Spot on the internal surface of the membrana vitelli in the Frog.
e. Membrana vitelli.
f. Chorion,—also ovum.
g. Peculiar granules, first constituting the envelope of the germinal vesicle, next contained in the fluid of the ovisac, and lastly arranging themselves to form:
  g^1. Tunica granulosa.
  g^2. Retinacula.
  g^3. Membrana granulosa.
h. Ovisac, or proper membrane of the ovisac.
i. Proper covering or tunic of the ovisac,—also blood-vessels.
  h i. Graafian vesicle, or corresponding structure (capsule) (consisting of $h + i$).
k. Stroma, Parenchyma, substance in which the ovisac is imbedded, or couch.
l. Peritoneum.
  i k l. Pedicle (formed by the union of *i*, *k* and *l*).
xx. "Stigmate" of Baer.
* Oil-like globules.
  g^*. Isolated spots (consisting chiefly of $g + *$).

PLATE V.

Fig. 1. Rabbit (*Lepus Cuniculus*, Linn.). Very early stages in the formation of the ovum (14.). *c.* The germinal vesicle containing (*a*) fluid, and in one instance (*b*) a well-marked spot. These germinal vesicles measured from $\frac{1}{150}'''$ to $\frac{1}{50}'''$ (Paris line) in their long diameter. *g.* An envelope of peculiar granules (14.). The ovisac is not yet formed. Compare with fig. 2. from the Cat, and figs. 19. and 20. from the Pigeon. 440 *diameters*.

Fig. 2. Cat (*Felis Catus*, Linn.). Stages in the formation of the ovum (16.) nearly corresponding to some of those seen in fig. 1., but the germinal vesicle in this instance is concealed (12. 24.). *g.* Envelope of peculiar granules;

*. oil-like globules added to the envelopc of granules: these objects measured $\frac{1}{75}'''$ (Paris line) in length. 440 *diam.*

Fig. 3. Cat (*Felis Catus*, Linn.). Stages more advanced (16.). These objects measured $\frac{1}{50}'''$ (Paris line) in length. 440 *diam.*

Fig. 4. Ox (*Bos Taurus*, Linn.). More than a hundred parasitic ovisacs (*h*) (57.) in the field of vicw $\frac{1}{60}'''$ to $\frac{1}{25}'''$ (Paris line) (4.). In several of these ovisacs the germinal vesicle is visible. 100 *diam.*

Fig. 5. Sheep (*Ovis Aries*, Linn.). Ovisacs $\frac{1}{30}'''$ to $\frac{1}{20}'''$ (Paris line); one larger, $\frac{1}{5}'''$, containing an ovum. *k*. Stroma. 60 *diam.*

Fig. 6. Cat (*Felis Catus*, Linn.). Ovisacs $\frac{1}{50}'''$ (Paris line) and upwards. 100 *diam.*

Fig. 7. Ditto. Some of the ovisacs seen in the last figure more highly magnified. *g*. Peculiar granules; *b*. germinal spot; *c*. germinal vesiclc.

Fig. 8. Ditto. Two of the same ovisacs still more magnified. 440 *diam.*

Fig. 9. Dog (*Canis familiaris*, Linn.). Ovisacs $\frac{1}{100}'''$ and upwards (in outline only). *g*. Peculiar granules, nearly as large as some of their containing ovisacs; *h*. proper membrane of thc ovisac, very thick, and presenting folds in its contour (2. 7.). 440 *diam.*

Fig. 10. Cat (*Felis Catus*, Linn.). Parasitic ovisacs (57.) $\frac{1}{50}'''$ to $\frac{1}{17}'''$. *h*. Proper membrane of the ovisac (7.); *g*. peculiar granules; *c*. germinal vesicle. Compare with fig. 21. from the Pigeon, and fig. 32. Plate VI. from the Whiting. 240 *diam.*

Fig. 11. Man. Ovisac $\frac{1}{25}'''$. *g*. Peculiar granules; *h*. proper membrane of the ovisac. 440 *diam.*

Fig. 12. Ditto. Ovisacs $\frac{1}{25}'''$, and upwards; the minuter ones more spherical (3.) than the one in the last figure. The germinal vcsicle (*c*) is visible in the largest of the ovisacs. 150 *diam.*

Fig. 13. Cat (*Felis Catus*, Linn.). Parasitic ovisacs (57.): the smallest $\frac{1}{50}'''$, the largest $\frac{1}{25}'''$. *b*. Germinal spot; *c*. germinal vesicle; *. oil-like globules; *h*. proper membrane of the ovisac. 440 *diam.*

Fig. 14. Hog (*Sus Scrofa*, Linn.). Ovisac $\frac{1}{25}'''$. *. Oil-like globules surrounding the minute germinal vesicle; *g*. peculiar granules; *h*. proper membrane of thc ovisac. 240 *diam.*

Fig. 15. Ditto. Incipient yelk (oil-like globules (*) and fluid). The germinal vesicle is concealed. *g*. Peculiar granules; *h*. proper membrane of the ovisac. Compare with figs. 22. 23. and 24. from Birds. 240 *diam.*

Fig. 16. Ditto. Membrana vitelli (*e*) partly visible. *. Oil-like globules, some of which closely surround and conceal the minute germinal vesicle; *h*. ovisac. Compare with fig. 25. from the common Fowl; fig. 34. Plate VI. from the Turbot; and fig. 35. Plate VI. from the Salmon. 240 *diam.*

Fig. 17. Cat (*Felis Catus*, Linn.). Thick and transparent chorion (*f*) just formed (52—56.). *e*. Membrana vitelli almost concealed by the chorion (the

separate existence of the former is seen in two places (49.)); *b.* germinal spot; *c.* germinal vesicle; *. oil-like globules in the yelk, which consisted besides of a pellucid fluid and minute granules; *g.* peculiar granules; *h.* ovisac. No appearance yet of either the tunica granulosa (64.), the retinacula (80.), the membrana granulosa (72.), or the proper covering of the ovisac (77.). 440 *diam.*

Fig. 18. Pigeon (*Columba livia*, Linn.). A very early stage in the formation of the ovum (15.). *c.* Germinal vesicle; *a.* its contents; *. oil-like globules†. Compare with fig. 30. Plate VI. from the Salmon. 440 *diam.*

Fig. 19. Ditto. This figure corresponds to fig. 1. from the Rabbit, and fig. 2. from the Cat (15.). *g.* Envelope of peculiar granules; *c.* germinal vesicle; *. oil-like globules†. 440 *diam.*

Fig. 20. Ditto. Proper membrane of the ovisac (*h.*) in the course of formation around (*g.*) the envelope of peculiar granules (23.). This appears to be the stage succeeding that represented by the larger objects in fig. 1. from the Rabbit, and by those in fig. 3. from the Cat†. 440 *diam.*

Fig. 21. Ditto. A multitude of ovisacs. Compare with fig. 10. from the Cat, and fig. 32. Plate VI. from the Whiting. 150 *diam.*

Fig. 22. Ditto. Incipient yelk (24.), consisting of oil-like globules (*) and a pellucid fluid. *c.* Germinal vesicle; *h.* ovisac. Compare with fig. 15. from the Hog. 440 *diam.*

Fig. 23. Common Fowl (*Phasianus Gallus*, Linn.). Incipient yelk (24.), consisting of oil-like globules (*) and a pellucid fluid. *c.* Germinal vesicle; *g.* peculiar granules; *h.* ovisac, $\frac{1}{20}'''$. Compare with fig. 15. from the Hog. 440 (?) *diam.*

Fig. 24. Ditto. The yelk (*d*) just formed (consisting of oil-like globules and a fluid), but the membrana vitelli not visible. *h.* Ovisac, $\frac{1}{17}'''$. The fluid of the ovisac is obscurely granulous. 440 *diam.*

Fig. 25. Ditto. Membrana vitelli (*e*) now visible. The yelk (*d*) consists still for the most part of oil-like globules and a pellucid fluid. *h.* Ovisac. Compare with fig. 16. from the Hog; fig. 34. Plate VI. from the Turbot; and fig. 35. Plate VI. from the Salmon. 440 *diam.*

Fig. 26. Ditto. An ovisac acquiring a proper covering or tunic, the union constituting the structure *h i* (47.), still very transparent. The yelk is seen escaping through the pedicle, which in this instance it has ruptured (32 Note). *e.* Membrana vitelli; *g.* peculiar granules. 100 *diam.* See the ideal section Plate VI. fig. 38½.

Fig. 26½. Cat (*Felis Catus*, Linn.). Oil-like globules (*) accumulated on the peculiar granules (*g*) of the ovisac. (Plate VIII. fig. 66. presents the same objects in less number, and therefore appearing as isolated spots, *g**).

† The objects represented in figs. 18. 19. and 20. were found lying among ovisacs apparently just formed.

PLATE VI.

Fig. 27. Frog (from R. WAGNER). *h.* Ovisac, $\frac{1}{50}'''$, ("chorion" of WAGNER (34.)); *b.* germinal spots, *many* in the *naked* Amphibia (R. WAGNER); *c.* germinal vesicle; *e.* membrana vitelli; *d.* yelk, pellucid at this period.

Fig. $27\frac{1}{2}$. *Coluber Natrix* (from R. WAGNER). *b.* Germinal spot, *single* in the *squamous* Amphibia (R. WAGNER); *c.* germinal vesicle; *d.* yelk, in its membrane; *h.* ovisac ("chorion" of WAGNER (34.)).

Fig. 28. Frog (*Rana temporaria*, LINN.). A spot (*d'*), generally elliptical, and in this instance $\frac{1}{25}'''$ in length, which I always find on the internal surface of the membrana vitelli of the Frog in ovisacs of about this size ($\frac{1}{5}'''$) (40.). *b.* Germinal spots; *c.* germinal vesicle; *d.* yelk; *e.* membrana vitelli; *h.* ovisac. 100 *diam.*

Fig. 29. Ditto. The membrana vitelli (*e*) rendered visible by maceration†. *c.* The germinal vesicle, indistinctly seen in the centre of (*d*) the yelk; *e.* membrana vitelli; *h.* ovisac. 60 *diam.*

Fig. 30. Salmon (*Salmo Salar*, LINN.). A very early stage in the formation of the ovum, nearly corresponding to figs. 18. and 19. Plate V. from the Bird, and to figs. 1. 2. and 3. Plate V. from Mammals (17.). *c.* Germinal vesicles, the largest $\frac{1}{30}$th of a Paris line in diameter; their contents consisting chiefly of minute oil-like globules of a yellowish colour. Compare these vesicles with the germinal vesicles in fig. 35. from the same fish. One of the germinal vesicles in fig. 30. has another vesicle external to it. Is this a minute ovisac just formed? (See the manner of origin of the ovisac (23.)). 240 *diam.*

Fig. 31. Cod (*Gadus Morrhua*, LINN.). Ovisacs, $\frac{1}{40}'''$ to $\frac{1}{10}'''$; the contour in folds (34.). The yelk is not yet visible, being pellucid at this period. *b.* Germinal spots, *many* in osseous Fishes (R. WAGNER); *c.* germinal vesicle, its fluid tinged with yellow; *. oil-like globules; *h.* ovisac. 100 *diam.*

Fig. 32. Whiting (*Gadus Merlangus*, LINN.). Ovisacs $\frac{1}{7}'''$ to $\frac{1}{5}'''$; the contour in folds (34.). *c.* Germinal vesicle; *g.* peculiar granules in the fluid of the ovisac (8.); *h.* proper membrane of the ovisac; *d.* incipient yelk (24.). 100 *diam.*

Fig. 33. Haddock (*Gadus Æglefinus*). The yelk (*d*) now visible as well as its proper membrane (*e*). *b.* Germinal spots; *c.* germinal vesicle; *h.* proper membrane of the ovisac. 100 *diam.*

Fig. 34. Turbot (*Pleuronectes maximus*, LINN.). Membrana vitelli (*e*) now visible. *c.* Germinal vesicle; *h.* ovisac; *d.* the yelk, consisting of oil-like globules immediately around the germinal vesicle, and of granules on the internal

† As recommended by R. WAGNER.

surface of the membrana vitelli. Compare with fig. 25. Plate V. from the common Fowl, and fig. 16. Plate V. from the Hog. 240 *diam.*

Fig. 35. Salmon (*Salmo Salar*, LINN.). Ovisacs and their contents in nearly the same state as those in the last figure. *g.* Some of the peculiar granules (8.) are seen in the fluid of the ovisac (34.). Compare with fig. 25. Plate V. from the common Fowl, and fig. 16. Plate V. from the Hog.

Fig. 36. Ditto. The ovisac (*h*) acquiring a proper covering or tunic (*i*) (47.). In this instance two minute ovisacs are included by the covering (57.), their situation here being within the incipient pedicle. 100 (?) *diam.*

Fig. 37. Ditto. The same objects (*h i*) in a more advanced stage, but the covering still very transparent. The vesicles have been crushed, and the yelk is seen escaping through the pedicles, taking with it its proper membrane (*e*). 25 *diam.* See the ideal section, Plate VI. fig. 38½.

Fig. 38. Ditto. The perfect calyx with its pedicle. 25 *diam.*

Fig. 38½. Bird, &c. Ideal perpendicular section of that portion of the ovary which is called the calyx, and its contents. *d.* The yelk surrounded by its proper membrane, *e*; *h.* lining membrane of the calyx. (This lining membrane of the calyx was originally the independent ovisac.) *i.* Vascular covering or tunic of *h.* (By the union of *h* and *i* there is constituted *h i*, that portion (capsule) of a calyx which is analogous to the Graafian vesicle of Mammals.) *k.* Substance of the ovary; *l.* peritoneum; *i*, *k*, and *l* are reflected off from *h*, thus forming a pedicle *i k l*. This pedicle is comparatively hollow, so that when the calyx is crushed, a portion of the yelk (meeting with least resistance at the point where the reflection above-mentioned takes place,) escapes into it.

Fig. 39. Dog (*Canis familiaris*, LINN.). Granules (*g*) of the Graafian vesicle circumscribed by a distinct line (central portion of the retinacula?). g^*. Isolated spots; g^3. membrana granulosa; g^1. tunica granulosa; *h i*. Graafian vesicle. 60 *diam.*

Fig. 40. Mole (*Talpa europæa*, LINN.). The same objects with the exception of g^*.

Fig. 41. Ditto. The tunica granulosa (g^1) in the course of formation (68.). *h.* Ovisac; *i.* blood-vessels; *b.* germinal spot (of a yellowish colour); *c.* germinal vesicle; *d.* yelk; *f.* chorion. 440 *diam.*

Fig. 42. Ditto. The tunica granulosa (g^1) removed entire from the Graafian vesicle. *c.* Germinal vesicle in the centre of (*d*) the yelk; *f.* chorion.

Fig. 43. Sheep (*Ovis Aries*, LINN.). The tunica granulosa removed almost entire from the ovisac. *b.* Germinal spot; *c.* germinal vesicle; *d.* yelk; *f.* chorion. 240 *diam.*

Fig. 44. Dog (*Canis familiaris*, LINN.). The tunica granulosa (g^1) very highly magnified; most of its granules presenting very distinctly a nucleus, and some of them being very globular in form (9.). *d.* Yelk; *f.* chorion. At

one part of the surface of the yelk is seen a pellucid space, which is the situation of the germinal vesicle. This figure presents the state of the ovum when absorption is commencing (60. Note.). 440 *diam.*

Fig. 45. Hog (*Sus Scrofa*, Linn.). The tunica granulosa (g^1) removed entire from the ovisac, with some adherent granules, which form part of the ruptured retinacula (g^2). *b.* Germinal spot; *c.* germinal vesicle; *d.* yelk; *f.* chorion. 150 *diam.*

Fig. 46. Ditto. The tunica granulosa (g^1) relatively thicker than the last, from a loosening of its granules by incipient liquefaction (g^2). Adherent granules of the ruptured retinacula. A pellucid space is seen at one part of the surface of (d) the yelk, which is the situation of the germinal vesicle. This figure presents an early stage in the absorption of the ovum (60. Note.).

Fig. 47. Dog (*Canis familiaris*, Linn.). The tunica granulosa (g^1) removed entire from the ovisac, with part of the ruptured retinacula (g^2) adherent. *d.* yelk; *f.* chorion. 150 *diam.*

PLATE VII.

Fig. 48. Dog (*Canis familiaris*, Linn.). Incipient separation of the granules from the fluid of the ovisac (78.). g^2. First appearance of the retinacula; *d.* yelk, the only portion of the ovum in this instance visible from the exterior of (h i) the Graafian vesicle. 60 *diam.*

Fig. 49. Guinea Pig (*Cobaya Aperea*, Cuv.). The same objects in a stage more advanced (78.). g^1. Tunica granulosa. 150 *diam.*

Fig. 50. Rabbit (*Lepus Cuniculus*, Linn.). A stage still more advanced (79.). The peculiar granules of the Graafian vesicle (h i) are now seen to have become arranged into three incipient structures, viz. g^1. the tunica granulosa (64.); g^2. the retinacula (80.); g^3. the membrana granulosa (72.); *f.* the chorion. 100 (?) *diam.*

Fig. 51. Sheep (*Ovis Aries*, Linn.). The retinacula (g^2) supporting the ovum (f) in the centre of (h i) the Graafian vesicle (80.). The tunica granulosa, though probably formed, is not distinctly visible. 60 *diam.*

Fig. 52. Ox (*Bos Taurus*, Linn.). The same objects, together with (g^1) the tunica granulosa. 100 *diam.*

Fig. 53. Ditto. The retinacula (g^2) removed from the Graafian vesicle. g^1. Tunica granulosa; *f.* chorion (ovum). 100 *diam.*

Fig. 54. Dog (*Canis familiaris*, Linn.). The same objects, though in this figure the membrane of the central portion of the retinacula (g^2) is closely applied to, and hence scarcely distinguishable from, the tunica granulosa (g^1) (88. Note). *c.* Germinal vesicle in the centre of the yelk. 100 *diam.*

Fig. 55. Ox (*Bos Taurus*, Linn.). The ovum (f) approaching the periphery of

(*h i*) the Graafian vesicle, being conveyed by (g^2) the retinacula. g^1. Tunica granulosa ; g^3. membrana granulosa. Contrast with fig. 62. Plate VII. and with fig. 80. Plate VIII. from BAER (62. 63. 91.). The "cumulus" of BAER (central portion of the retinacula) is well seen in this figure. 60 *diam.*

Fig. 56. Ox (*Bos Taurus*, LINN.). The ovum (*f*) (in its tunica granulosa) at the periphery of (*h i*) the Graafian vesicle, being retained there by (g^2) the retinacula (86.). The other objects as in the last figure. It is the *under* surface of the ovum, &c. that is here seen. 60 *diam.*

Fig. 57. Dog (*Canis familiaris*, LINN.). The ovum (*f*) apparently just reaching the periphery of (*h i*) (a portion of) the Graafian vesicle. g^2. Retinacula; g^1. tunica granulosa; g^3. membrana granulosa, part of which has been removed (86.). 100 *diam.*

Fig. 58. Ferret (*Mustela Furo*, LINN.). The ovum (*f*) at the periphery of (*h i*) the Graafian vesicle. g^2. Retinacula (80.) ; g^1. tunica granulosa ; *c*. germinal vesicle. 100 *diam.*

Fig. 59. Rabbit (*Lepus Cuniculus*, LINN.). The same objects. g^3. Membrana granulosa, part of which has been removed (86.).

Fig. 60. Ox (*Bos Taurus*, LINN.). The under surface of the central portion of the retinacula (g^2) ("*cumulus*" of BAER) at the periphery of (*h i*) the Graafian vesicle. *k*. Adherent stroma.

Fig. 61. Ditto. Fragment of the central portion of the retinacula (g^2), from which the granulous bands have been broken off, yet still containing the ovum in its tunica granulosa (g^1); the latter adherent to the walls of the cavity in which it lies. (The drawing was taken as the object lay under the microscope in a watch-glass.)

Fig. 62. From BAER, "Vésicule de DE GRAAF (24. 25.) (de grandeur moyenne) d'une truie, grossie dix fois; dissequée suivant son axe.

1. Surtout péritonéal (*l*).
2. Tissu cellulaire (le *stroma*) (*k*).
3. Couche externe (*i*)
4. Couche interne (*h*)
   x. x. Stigmate

   } de la capsule de la vésicule de DE GRAAF.

5. Membrane granuleuse (g^3)
6. Fluide contenu
7. Disque proligère (g^2)
8. Ovule (*f*)

   } du noyau de la vésicule de DE GRAAF."

This figure, from BAER, is lettered so as to correspond with the other figures.

Fig. 63. Rabbit (*Lepus Cuniculus*, LINN.). The retinacula (g^2), with their membrane, removed from the Graafian vesicle. g^1. Tunica granulosa, with its tail-like appendages (65.) ; *f*. chorion (ovum) ; *d*. yelk ; *c*. germinal vesicle ; *b*. germinal spot. 240 *diam.*

PLATE VIII.

Fig. 64. Hog (*Sus Scrofa*, Linn.). The ovum in its tunica granulosa (g^1) at the periphery of (*h i*) the Graafian vesicle, retained in this situation by (g^2) the retinacula (86.). 100 *diam.*

Fig. 65. Mouse (*Mus Musculus*, Linn.). The same objects; but the ovum (*f*) in this instance has not reached the periphery of (*h i*) the Graafian vesicle. *c*. The germinal vesicle, relatively very large; *b*. the germinal spot.

Fig. 66. Dog (*Canis familiaris*, Linn.). Four ova in one Graafian vesicle (88. Note), and at or near to its periphery. g^2. Retinacula; the central portion of the retinacula (g^2) reduced to little more than a membrane, which closely surrounds (g^1) the tunica granulosa, and appears to be the cause of the well-defined contour of the latter, as seen *in situ* in this animal (88. Note). g^*. Isolated spots (59.), consisting chiefly of extremely minute oil-like globules accumulated on the peculiar granules of the Graafian vesicle. In this instance these spots were seen to be most numerous around some of the ova. (Plate V. fig. 26½. presents these spots in close aggregation.)

Fig. 67. Rabbit (*Lepus Cuniculus*, Linn.). An appearance frequent in this animal, probably denoting a stage in the absorption of the ovum (60.). *f*. Distended chorion; *d*. altered yelk; *c*. the enlarged germinal vesicle; *b*. the enlarged germinal spot; *h i*. Graafian vesicle. (The tunica granulosa and retinacula have disappeared.) 100 *diam.*

Fig. 68. Dog (*Canis familiaris*, Linn.). A patch of ovisacs (*h*) $\frac{1}{30}'''$ to $\frac{1}{5}'''$ in length, some of them passing into the state of Graafian vesicles, and among these two (*h i*) that have become opake from the coverings acquired. One of the ovisacs contains two ova (88. Note). *g*. Granules of the ovisac (incipient retinacula?).

Fig. 69. Sheep (*Ovis Aries*, Linn.). The cavity in which the ovisac is often found (6.). The ovisac (*h*) having been ruptured, the ovum (*f*) has escaped, and is seen to have become flattened by pressure. The ovum bursting before its exit, left behind its germinal vesicle (*c*). *b*. Germinal spot; *g*. peculiar granules of the ovisac; *k*. substance in which the ovisac is imbedded. 60 *diam.*

Fig. 70. Cat (*Felis Catus*, Linn.). The chorion (*f*) not yet of such consistence as to resist even very gentle pressure, which, applied on one side, has forced the now semifluid substance of the chorion to the other side (53.). *e*. Membrana vitelli (49. 53.); *d*. the yelk in an incipient state. 440 *diam.*

Fig. 71. Hog (*Sus Scrofa*, Linn.). Portion of the membrana granulosa (g^3) removed from the Graafian vesicle (73.). 100 *diam.*

Fig. 72. Man (from Bernhardt). An ovum after maceration (50. 56.). *d*. The yelk; *f*. the chorion. The internal surface of the latter is the "mem-

brana ovuli externa" of Bernhardt, and its transparent substance is the "spatium pellucidum" of this author. g^1. Ruptured tunica granulosa, the "zona granulosa" of Bernhardt (Symbolæ, &c., fig. xxiii. pp. 45. 46.).

Fig. 73. Ox (*Bos Taurus*, Linn.). *g*. Some of the peculiar granules of the ovisac with their nucleus. In one instance a single granule presents two nuclei (8. 10.). This granule measured $\frac{1}{750}$th Paris line in length. 800 *diam.*

Fig. 74. Hog (*Sus Scrofa*, Linn.). A patch of parasitic ovisacs (*h*) (57.) in (*h i*) the parietes of a Graafian vesicle. There is seen in several a tapering at one end (3.). They are all more spherical than when first formed (3.). *f*. The ovum. 60 *diam.*

Fig. 75. Dog (*Canis familiaris*, Linn.). Part of a Graafian vesicle (*h i*), with parasitic ovisacs (57.) (*h*) in its parietes. Compare with fig. 76. from the Pigeon. 100 *diam.*

Fig. 76. Pigeon (*Columba livia*, Linn.). Ovisac with its vascular covering; the union of these two presenting (*h i*) a structure analogous to the Graafian vesicle of Mammals (47.). In the parietes of this structure, and probably *included* (57.) on the acquisition by the ovisac of its covering, are several parasitic ovisacs (*h*). The vesicle has been crushed, but the minute parasitic ovisacs remain unmoved. The yelk has escaped. *g*. Peculiar granules passing slowly out of the vesicle. Compare these with the corresponding granules in Mammalia (8—10.); and compare the parasitic ovisacs in this figure with those in fig. 75. from the Dog. 60 *diam.*

Fig. 77. Ox, Heifer (*Bos Taurus*, Linn.). Ovary, with the outer third of a Graafian vesicle reflected. In the parietes of the Graafian vesicle there is seen transversely divided (*h*) a parasitic ovisac (58.), or incipient Graafian vesicle. *Actual size.*

Fig. 78. Ditto. Enlarged view of the two halves of the divided parasitic ovisac, or incipient Graafian vesicle, seen in fig. 77. *f*. Ovum; g^3. membrana granulosa; *h*. proper membrane of the parasitic ovisac.

Fig. 79. Ditto. Transverse section of the proper membrane (*h*) of the parasitic ovisac, or incipient Graafian vesicle, shown in figs. 77. and 78, here removed from its proper cavity (6.). The membrana granulosa (g^3) has fallen down. 25 *diam.*

Fig. 80. (from Baer). "Ovule avec le disque proligère (g^2) d'une vache, grossie dix fois." Contrast with fig. 55. Plate VI.


1 Rabbit.
18. Pigeon.
10 Cat.
21 Pigeon.
2 Cat.
19 Pigeon.
3 Cat.
20 Pigeon.
11 Man.
4 Ox.
12 Man.
22 Pigeon.
5 Sheep.
13 Cat
15 Hog.
23 Common Fowl.
24 Common Fowl.
16 Hog.
25 Common Fowl.
6 Cat.
7 Cat
14 Hog.
8 Cat.
26 Common Fowl.
26½ Cat
17 Cat.
9 Dog.

Embryology.

Embryology.

Ad Nat. del. Martin Barry. F. Basire, sc.


48 Dog.
49 Guineapig.
51 Sheep.
50 Rabbit.
52 Ox.
53 Ox.
63 Rabbit.
54 Dog.
57 Dog.
55 Ox.
59 Rabbit.
61 Ox.
60 Ox.
58 Ferret.
62 Hog (According to Baer)
56 Ox.

Embryology.

Ad Nat. del. Martin Barry. Embryology. J^s Basire. sc.

XVI. *Magnetical Observations made in the West Indies, on the North Coast of Brazil and North America, in the Years* 1834, 1835, 1836, *and* 1837, *by Captain Sir* Everard Home, *Bart. R.N. F.R.S. Reduced by the Rev.* George Fisher, *M.A. F.R.S.*

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It was the object in the experiments recorded in this paper, to determine the relative magnetic forces soliciting both the dipping, and horizontal needles, by observing the times of their completing a given number of vibrations at the various places visited during a period of three years, on the North American and West India Station, in Her Majesty's Ship Racehorse.

The dipping instrument used was one of modern construction by Dollond. Each observation for the dip consisted of an equal number of readings of the positions of the needle, with the face of the instrument east and west, before and after the inversion of the poles, and a mean of all the readings taken for the true dip. The instrument had two needles fitted to it, one of which being used solely for the purpose of observing its vibrations, its magnetism was therefore never interfered with, and this needle in this paper is distinguished by the letter B. The other needle was kept for the purpose of determining the dip, and the results obtained with it are given in Table I.

For the purpose of determining the horizontal forces, besides the needle B, four other needles, Nos. 1, 2, 3, and 4, were used, the first two, four and a half, the others, three inches in length, and of a cylindrical form. When used they were suspended by fibres of raw silk in an apparatus constructed for the purpose. They had fixed centres of suspension, with sliding weights for their horizontal adjustments. The vibrations were confined to the same limits of arc, so as to render the experiments as comparative as possible, and to avoid any correction for circular arc. The state of the thermometer was also registered, though at present it is impossible to assign with any degree of accuracy the correction due to the results arising from the differences of temperatures at which the experiments were made.

The needles Nos. 1, 2, 3, and 4 appear, by comparing the experiments made near London previous to the voyage, with similar ones at the same place at the expiration of it, to have lost during the interval some portion of their magnetism; the same circumstance is also observable from the experiments made at Bermuda and other places. To compensate for this change in the intensities of the needles, the experiments contained within the horizontal lines in the Tables in which they are recorded

have been reduced to the same epoch, or mean period of observation, by the application of a proportional part of this change; and the corrected or concluded times of completing a given number of vibrations for each needle, together with the corresponding forces, which are inversely as the squares of these times, are given in separate columns. These forces are again reduced, and compared with the horizontal force at Plymouth, which is taken equal to unity.

The magnetism of the needle B appears to have been subject to some occasional changes during the voyage, but not of sufficient magnitude or regularity as to admit of any correction being made on this account. The mean of all the experiments collectively, that were made at the same place with this needle, is therefore taken as one result, and compared with others obtained in a similar way at the other places.

Some degree of uncertainty is attached to experiments of this nature when made with various needles, the whole of which have only been occasionally used, and the magnetism of these subject to various changes, on account of the difficulty of assigning such values for the final results as the experiments themselves seem best to justify, since the means as deduced from the experiments at each place are on this account somewhat vitiated, being no longer *strictly* comparative; and moreover the same experiments may by different persons be differently compared with each other, and thereby results somewhat different may be obtained.

It is necessary to state, therefore, that the horizontal forces at Plymouth, Bermuda, and Port Royal in Jamaica have been determined by the experiments made at Plymouth in March 1834, immediately before leaving England, compared with similar ones made at the two latter places in the months of June and July following, and by applying a small correction for the diminution in the intensities of the respective needles. The experiments at Chagres are compared with the previous and subsequent ones at Jamaica made at short intervals of times; those at Parà, Maranham, and Demerara with those made at Bermuda; and lastly those at Antigua with the previous and subsequent ones at Bermuda. The experiments thus grouped and compared together are contained between the horizontal lines in the Tables II., III., IV., and V., together with the comparative horizontal forces between these places; these forces are then compared with the force at Plymouth, and the results given in a separate column.

Having thus obtained the horizontal forces at each place as compared with Plymouth, these are again reduced to the forces in the direction of the dipping needle by multiplying the horizontal forces by the fraction $\frac{\cos.\text{dip at Plymouth}}{\cos.\text{dip at Place}}$. The comparative whole forces being deduced by means of the horizontal needles, and also by direct experiment with needle B when vibrated in the dipping instrument, the final results are given in Table VIII.

G. F.

Table I.

Containing the Dip as observed at different places.

Dates.	Places.	Lat.	Long.	Dip.	Remarks.
1833. July 20 and 29.	Near London . .	51° 26′ N.	0° 18′ W.	69° 52′ 38″	Observations made at Ham, near London.
1834. Jan. 4.	,,	,,	,,	70 17 42	Observations made at the same place.
Mar. 3.	Plymouth	50 22	4 9	69 58 2	At the Athenæum.
11.	,,	,,	,,	69 56·20	,,
April 2.	,,	50 20	4 7	69 44 54	On the Breakwater (in the centre).
9.	,,	,,	,,	69 41 10	,,
May 27.	Halifax	44 39	63 37	75 33 1	At the Observatory in the Dockyard.
June 11.	Bermuda	32 18	64 50	67 46 21	In the Dockyard.
16.	,,	,,	,,	67 44 12	,,
July 11.	Jamaica	17 55	76 50	47 0 42	On the Point at Port Royal.
11.	,,	,,	,,	47 5 7	,,
Aug. 1.	,,	,,	,,	47 4 41	,,
Sept. 11.	Nicaragua	10 58	83 43	34 2 54	Point Arenas.
12.	,,	,,	,,	34 7 42	
18.	Chagres	9 20	80 1	32 36 4	Castle of St. Lorenzo (on the platform).
18.	,,	,,	,,	32 31 5	
19.	,,	,,	,,	32 23 26	
30.	Jamaica	17 55	76 50	46 49 16	Port Royal Point.
Oct. 6.	,,	,,	,,	47 6 40	
1835. Feb. 6.	Alta Vela	17 28	71 39	47 38 32	
April 30.	Antigua	17 3	61 50	48 46 15	At the Fort in English Harbour.
May 11.	Barbadoes	13 5	59 37	43 45 56	Engineers' Wharf, Shot-hall, Carlisle Bay.
June 5.	Parà	1 26 S.	48 30	23 59 35	At the Consul's house (in the garden).
July 14.	,,	,,	,,	24 20 50	
26.	,,	,,	,,	23 56 52	
27.	,,	,,	,,	23 56 8	
Oct. 1.	Maranham	2 30	44 18	23 23 3	On Point Francisco.
5.	,,	,,	,,	23 30 37	,,
16.	,,	,,	,,	23 29 54	,,
16.	,,	,,	,,	23 39 41	,,
1836. Jan. 5.	Barbadoes	13 5	59 37	43 28 45	The same place as before.
Feb. 6.	Bermuda	32 18	64 50	67 17 42	,,
April 11.	City of Caraccas	,,	,,	37 16 25	
May 30.	Demerara	,,	,,	33 56 27	In the Governor's garden.
July 26.	Parà	1 26	48 30	23 31 40	
26.	,,	,,	,,	23 19 0	
1837. June 7.	Halifax	44 39	63 37	74 57 52	In the Dockyard (at the observatory).
29.	Plymouth	50 20	4 7	69 38 32	Breakwater (in the centre).
Oct. 6.	Near London . .	51 26	0 18	69 25 12	At Ham, near London.

Note.—The dips and vibrations were for the greater part repeated by Mr. Byron Drury, Midshipman of the Racehorse, at the same times that the above observations were made.

Table II.

Experiments with Needle No. I.

Places.	Dates.	Time of Day.	Therm. Fahr.	Observed Times of 300 horizontal vibrations.	Concluded Times of 300 vibrations at the places between the horizontal lines, when reduced to the same epoch.	Comparative horizontal intensities at the places between the lines.	Comparative horizontal intensities. The force at Plymouth being unity.
		h m	°	s	s		
Plymouth	1834. Mar. 17.	2 0 P.M.	60·2	1284·76	1284·76	1·000	1·000
Bermuda..	June 17.	2 30	88·0	1113·70	1108·50	1·343	1·343
Jamaica ..	July 19.	Noon	90·0	917·50	911·84	1·985	1·985
Jamaica ..	July 19.	Noon	90·0	917·50			
Chagres ..	Sept. 19.	1 46 P.M.	87·5	891·45	891·45	1·064	2·113
Jamaica ..	Oct. 3.	2 0	89·5	920·17	919·67	1·000	
Parà	1835. July 20.	2 0 P.M.	88·6	1009·81	1010·89	1·409	1·892
Maranham	Oct. 3.	3 0	91·5	1017·70	1017·10	1·391	1·869
Maranham	Nov. 9.	2 10	89·0	1015·83			
Bermuda..	1836. Feb. 7.	1 6	75·0	1199·83	1199·83	1·000	
Demerara	May 31.	1 49	85·0	997·75	997·40	1·447	1·943
Parà	Oct. 6.	4 8	94·0	1011·98			
Maranham	Nov. 18.	11 30 A.M.	92·0	1017·77			
Bermuda..	1834. June 17.	2 30 P.M.	88·0	1113·70			
Antigua ..	1835. April 30.	3 34	88·5	968·81	968·81	1·433	1·925
Bermuda..	1836. Feb. 7.	1 6	75·0	1199·83	1159·40	1·000	

Table III.

Experiments with Needle No. II.

Places.	Dates.	Time of Day.	Therm. Fahr.	Observed Times of 300 horizontal vibrations.	Concluded Times of 300 vibrations at the places between the horizontal lines, when reduced to the same epoch.	Comparative horizontal intensities at the places between the lines.	Comparative horizontal intensities. The force at Plymouth being unity.
		h m	°	s	s		
Plymouth	1834. Mar. 26.	2 30 P.M.	59·4	1190·06	1190·06	1·000	1·000
Bermuda..	June 17.	3 6	88·0	1033·60	1032·45	1·329	1·329
Jamaica ..	July 19.	1 0	90·5	853·43	852·12	1·950	1·950
Jamaica ..	July 19.	1 0	90·5	853·43			
Chagres ..	Sept. 19.	2 10	100·0	834·66	834·66	1·056	2·059
Jamaica ..	Oct. 3.	2 45	87·5	858·71	857·70	1·000	
Parà	1835. July 20.	3 0	90·8	881·93	884·16	1·405	1·866
Maranham	Oct. 3.	4 5	88·0	888·06	889·65	1·387	1·843
Maranham	Nov. 9.	3 25	88·0	887·85			
Bermuda..	1836. Feb. 7.	2 5	76·5	1047·53	1047·91	1·000	
Demerara	May 31.	2 22	83·0	872·50	871·38	1·446	1·922
Parà	Oct. 6.	5 0	86·5	886·40			
Maranham	Nov. 18.	1 0	95·0	893·04			
Bermuda..	1834. June 17.	3 6	88·0	1033·60	1040·98	1·000	
Antigua ..	1835. April 30.	4 14	84·0	893·47	893·47	1·357	1·804
Bermuda..	1836. Feb. 7.	2 5	76·5	1047·53			

Table IV.

Experiments with Needle No. III.

Places.	Dates.	Time of Day.	Therm. Fahr.	Observed Times of 300 horizontal vibrations.	Concluded Times of 300 vibrations at the places between the horizontal lines when reduced to the same epoch.	Comparative horizontal intensities at the places between the lines.	Comparative horizontal intensities. The force at Plymouth being unity.
		h m	°	s	s		
Plymouth	1834. Mar. 24.	3 0 P.M.	58·0	953·40	953·40	1·000	1·000
Bermuda . .	June 17.	3 40	88·0	832·80	832·07	1·313	1·313
Jamaica . .	July 19.	0 43	91·0	691·30	690·49	1·907	1·907
Jamaica . .	July 19.	0 43	91·0	691·30			
Chagres . .	Sept. 19.	2 38	99·0	670·58	670·58	1·067	2·035
Jamaica . .	Oct. 3.	4 0	93·0	693·20	692·84	1·000	
Parà	1835. July 23.	4 0	88·5	707·89	710·30	1·408	1·849
Maranham	Oct. 3.	4 38	86·5	712·25	711·92	1·402	1·840
Maranham	Nov. 9.	4 34	85·0	711·98			
Bermuda . .	1836. Feb. 7.	2 47	72·5	842·73	842·91		
Demerara	May 31.	3 0	83·0	701·22	700·57	1·447	1·901
Parà	Oct. 6.	5 34	84·0	712·71			
Maranham	Nov. 19.	1 34	94·5	711·53			
Bermuda . .	1834. July 17.	3 40	88·0	832·80	838·07	1·000	
Antigua . .	1835. Apr. 30.	5 32	79·5	665·60	665·60	1·585	2·081
Bermuda . .	1836. Feb. 7.	2 47	72·5	842·73			

Table V.

Experiments with Needle No. IV.

Places.	Dates.	Time of Day.	Therm. Fahr.	Observed Times of 300 horizontal vibrations.	Concluded Times of 300 vibrations at the places between the horizontal lines when reduced to the same epoch.	Comparative horizontal intensities at the places between the lines.	Comparative horizontal intensities. The force at Plymouth being unity.
		h m	°	s	s		
Plymouth	1834. Mar. 24.	4 50 P.M.	56·9	874·55	874·55	1·000	1·000
Bermuda . .	June 17.	4 20	89·0	759·98	757·90	1·331	1·331
Jamaica . .	July 19.	3 55	87·5	626·93	624·75	1·959	1·959
Jamaica . .	July 19.	3 55	87·5	626·93			
Chagres . .	Sept. 19.	3 36	95·0	610·08	610·08	1·075	2·107
Jamaica . .	Oct. 3.	5 15	85·7	633·92	632·59	1·000	
Parà	1835. July 23.	4 0	86·9	654·06	656·02	1·413	1·882
Maranham	Oct. 3.	5 2	83·5	659·65	660·65	1·394	1·856
,,	Nov. 9.	5 13	82·5	659·73			
Bermuda . .	1836. Feb. 7.	3 17	72·0	779·53	779·95	1·000	
Demerara	May 31.	3 45	82·5	649·08	647·33	1·452	1·933
Parà	Oct. 6.	6 4	84·0	657·98			
Maranham	Nov. 19.	2 47	90·0	661·95			

Table VI.

Experiments with Needle B. [Horizontal Vibrations.]

Places.	Dates.	Time of Day.	Therm. Fahr.	Observed Times of 200 horizontal vibrations.	Concluded Times of 200 horizontal vibrations.	Comparative horizontal intensities.
		h m	°	s	s	
Plymouth ..	1834. Mar. 26.	3 0 P.M.	59·7	915·67	915·67	1·000
Halifax	May 27.	Noon	50·7	977·95	973·09	0·885
Halifax	1837. June 7.	3 42 P.M.	64·0	968·23		
Bermuda....	1834. June 15.	2 20	88·2	794·98	801·90	1·304
Bermuda....	1836. Feb. 7.	2 0	84·0	808·82		
Jamaica	1834. July 16.	3 30	87·7	656·47	659·04	1·930
Jamaica	Oct. 1.	1 15	90·0	661·62		
Nicaragua ..	Sept. 11.	1 39	84·0	635·43	635·43	2·076
Chagres	Sept. 18.	Noon	100·0	639·80	639·68	2·049
Chagres	Sept. 19.	11 A.M.	84·0	639·56		
Alta Vela ..	1835. Mar. 6.	0 25 P.M.	92·0	686·40	686·40	1·780
Antigua	April 30.	3 30	89·2	688·33	688·33	1·770
Barbadoes ..	May 11.	2 0	86·5	677·51	677·51	1·827
Parà	July 23.	1 0	87·8	677·66	676·59	1·832
Parà	1836. Oct. 6.	5 0	100·0	675·57		
Maranham ..	1835. Oct. 21.	4 0	91·5	687·18	690·76	1·757
Maranham ..	1836. Nov. 18.	10 30 A.M.	93·0	694·34		
Demerara ..	May 31.	1 10 P.M.	85·0	674·00	674·00	1·846

Table VII.

Experiments with Needle B. [In the direction of the Dipping Needle.]

Places.	Dates.	Time of Day.	Therm. Fahr.	Observed Times of 40 vibrations in the direction of the dipping needle.	Concluded Times of 40 vibrations as before.	Comparative intensities in the direction of the dipping needle.
		h m	°	s	s	
Plymouth ..	1834. Mar. 24.	3 0 P.M.	56·0	112·52	111·49	1·000
Plymouth ..	1837. June 29.	3 0	84·0	110·45		
Halifax	1834. May 27.	3 0	52·0	98·05	97·60	1·306
Halifax	1837. June 7.	Noon	64·0	97·15		
Bermuda....	1834. June 13.	3 30 P.M.	93·5	100·49	102·34	1·187
Bermuda....	1836. Feb. 6.	3 30	72·0	104·20		
Jamaica	1834. July 11.	4 30	99·0	108·50	108·72	1·052
Jamaica	Oct. 6.	11 0 A.M.	86·0	108·95		
Nicaragua ..	Sept. 11.	2 0 P.M.	78·0	114·75	114·75	0·944
Chagres	Sept. 18.	Noon	92·0	117·57	117·57	0·899
Alta Vela ..	1835. Feb. 6.	Noon	113·0	116·80	116·80	0·911
Antigua	April 30.	3 0 P.M.	94·0	113·70	113·70	0·961
Barbadoes ..	May 11.	Noon	87·0	115·05	117·08	0·907
Barbadoes ..	1836. Jan. 5.	3 0 P.M.	85·0	119·10		
Demerara ..	May 30.	Noon	86·0	121·25	121·25	0·845
Parà	1835. July 9.	1 40 P.M.	95·0	126·97	126·97	0·771
Maranham ..	Oct. 7.	4 0	90·4	130·27	130·27	0·732

Table VIII.

Abstract of the Experiments.

Places.	Comparative horizontal intensities by each needle.						Comparative intensities in the direction of the dipping needle as deduced from		Means of the two last columns.	Mean dip at each place from Table I.
	No. 1.	No. 2.	No. 3.	No. 4.	B.	Means.	The horizontal needles.	The direct experiments with the dipping needle.		
Plymouth	1·000	1·000	1·000	1·000	1·000	1·000	1·000	1·000	1·000	69° 44′ 19″
Halifax ..	....	....	....	....	0·885	0·885	1·204	1·306	1·255	75 15 26
Bermuda..	1·343	1·329	1·313	1·331	1·304	1·324	1·203	1·187	1·195	67 36 5
Jamaica ..	1·985	1·950	1·907	1·959	1·930	1·946	0·988	1·052	1·020	47 1 15
Nicaragua	....	....	....	....	2·076	2·076	0·868	0·944	0·906	34 5 18
Chagres ..	2·113	2·059	2·035	2·107	2·049	2·073	0·851	0·899	0·875	32 30 10
Alta Vela	....	....	....	....	1·780	1·780	0·915	0·911	0·913	47 38 32
Antigua ..	1·925	1·804	2·081	....	1·770	1·895	0·996	0·961	0·978	48 46 15
Barbadoes	....	....	....	....	1·827	1·827	0·874	0·907	0·890	43 37 20
Parà	1·892	1·866	1·849	1·882	1·832	1·864	0·706	0·771	0·738	23 50 41
Maranham	1·869	1·843	1·841	1·856	1·757	1·833	0·692	0·732	0·712	23 30 49
Demerara	1·943	1·922	1·901	1·933	1·846	1·909	0·797	0·845	0·821	33 56 27

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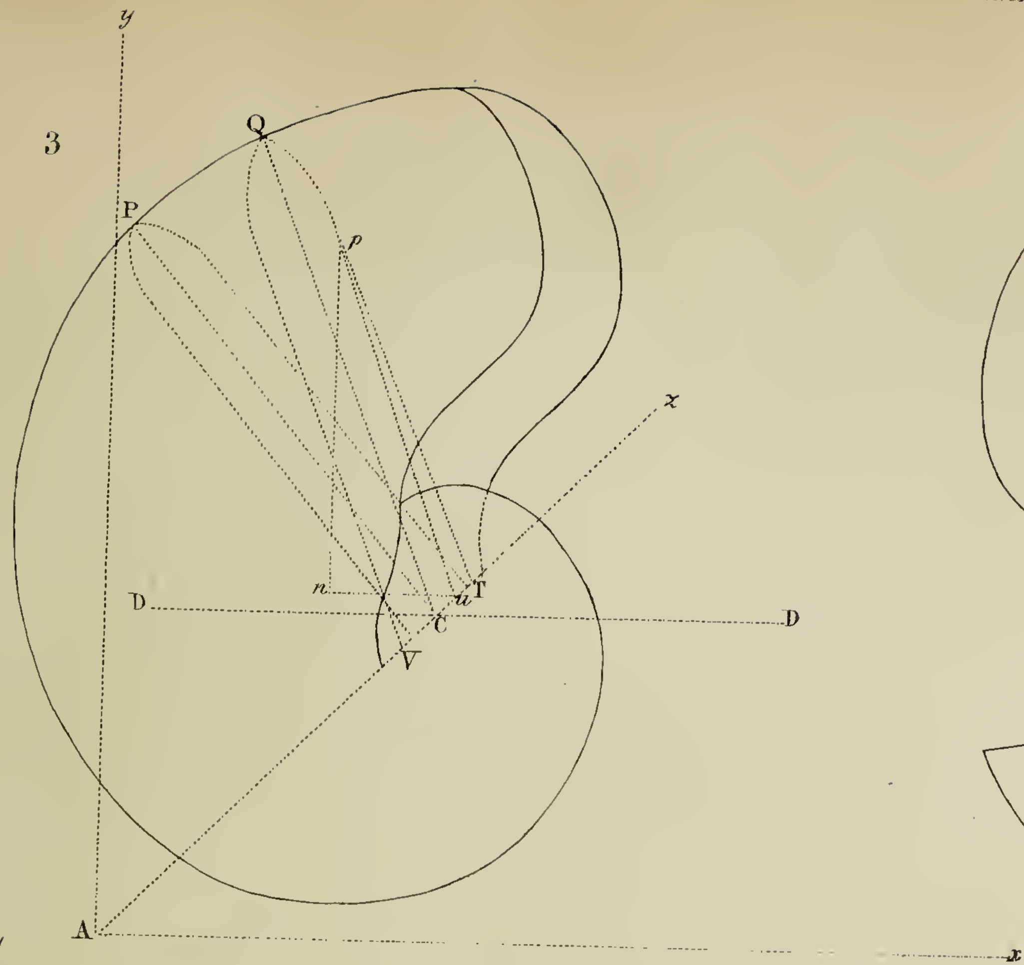
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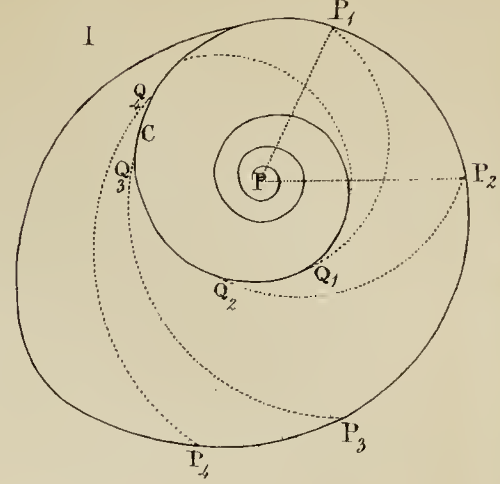
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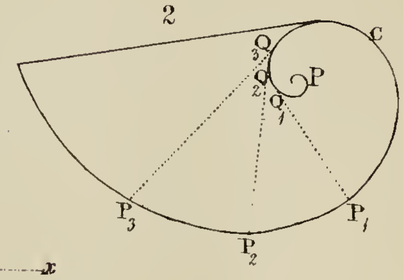
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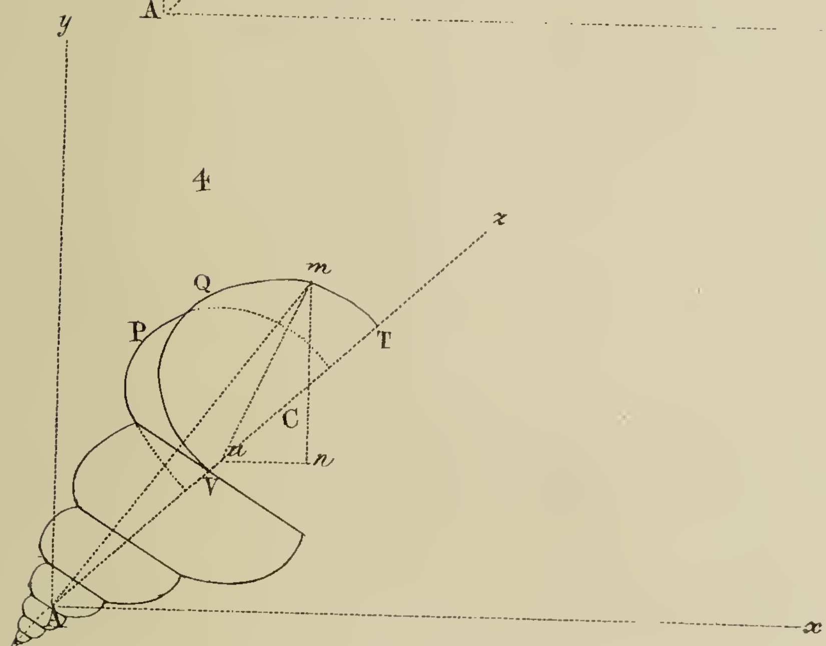
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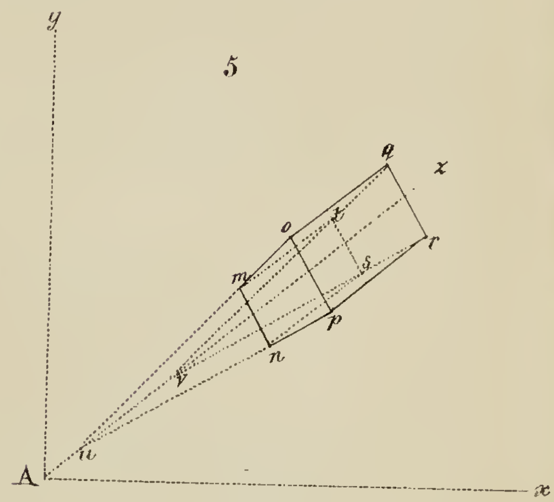
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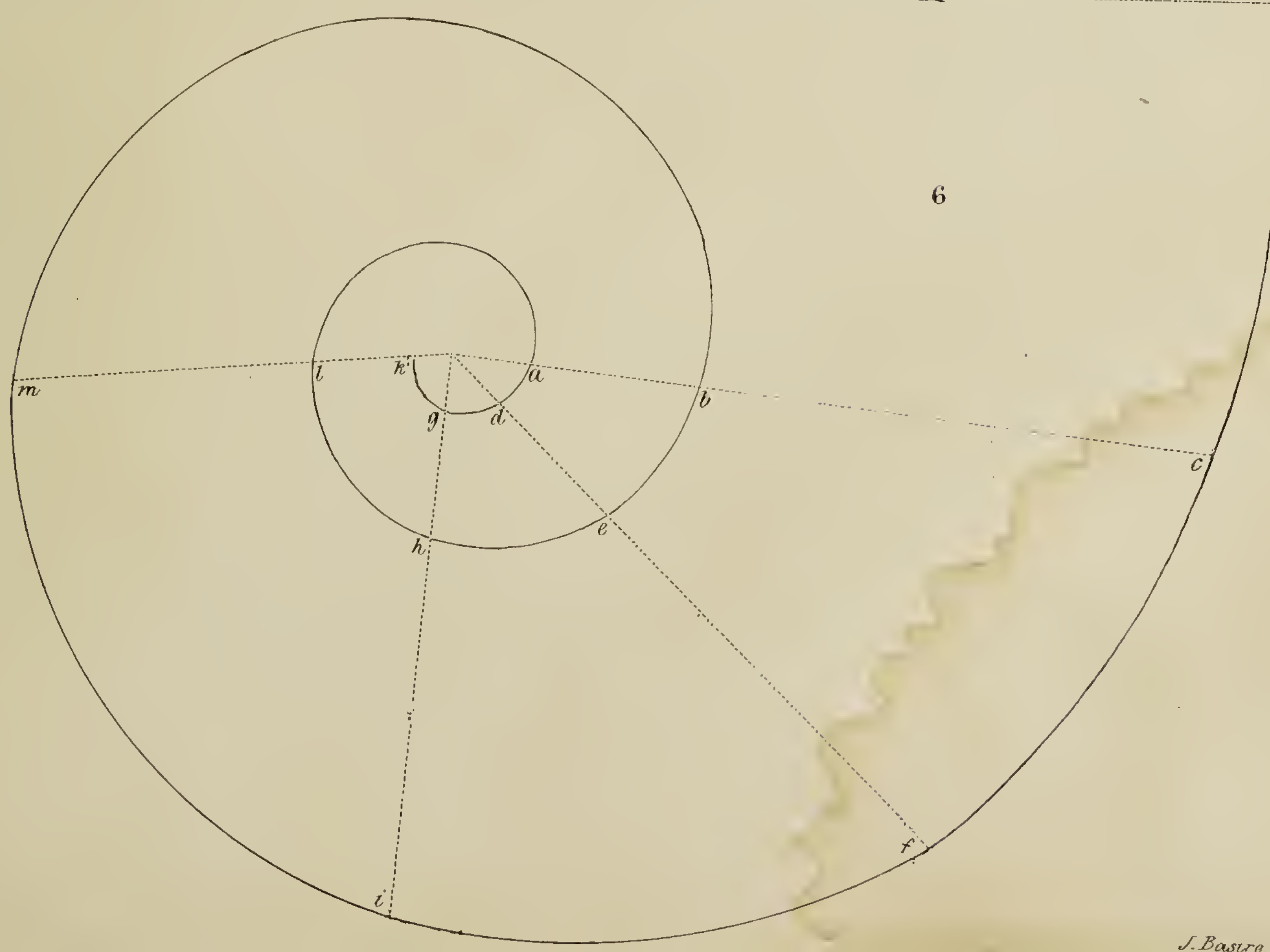
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XVII. *On the Geometrical Forms of Turbinated and Discoid Shells.*

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The surface of any turbinated or discoid shell may be imagined to be generated by the revolution about a fixed axis (the axis of the shell) of the perimeter of a geometrical figure, which, remaining always geometrically similar to itself, increases continually its dimensions.

In discoid shells the generating figure retains its position upon the axis as it thus revolves, as in the *Nautilus Pompilius* (Plate IX. fig. 3.), and the *Argonaut*. In turbinated shells, including the great families of Trochi, Turbines*, Murices and Strombi, it slides continually along the axis of its revolution (fig. 4.). In some great classes of shells, as the Ammonites, the *Nautilus scrobiculatus*, the *Nautilus spirula*, the *Helix cornea*, the *Trochus perspectivus*, the Nerita, the generating figure increases its distance from the axis at the same time that it increases its dimensions and revolves.

Among the generating figures of conchoidal surfaces are to be found various known geometrical forms. The generating figure of the *Conus Virgo* is a triangle, that of the *Trochus telescopicus* and of the *Trochus Archimedis*, a trapezoid. The species of the genus Turbo have for their generating figure a curve, of double curvature, of a circular or elliptic form, to whose perimeter the axis of revolution is a tangent. The *Nautilus Pompilius* is generated by the revolution about its shorter diameter of a plane curve, approaching very nearly to a semi-ellipse (fig. 3.); and the Cypræa by the revolution of a similar curve about its longer diameter.

There is a mechanical uniformity observable in the description of shells of the same species, which at once suggests the probability that the generating figure of each increases, and that the spiral chamber of each expands itself, according to some simple geometrical law common to all. To the determination of this law, if any such exist, the operculum lends itself, in certain classes of shells, with remarkable facility. Continually enlarged by the animal, as the construction of its shell advances, so as to fill up its mouth, the operculum measures the progressive widening of the spiral chamber, by the progressive stages of its growth.

Of these progressive stages of the growth of the operculum, distinct traces remain

* The beautiful shell *Turbo scalaris* (Ventletrap) may be taken as an easy illustration of the properties to be described in this paper.

on its surface, under the form in the Turbines (fig. 1.) of certain *curved* lines, and in the Neritæ (fig. 2.) of certain *straight* lines, passing from the margin of the operculum (which if produced they would intersect) to a certain spiral line marked deeply upon its face. To this spiral they are tangents, and may be supposed to generate it by their consecutive intersections. The spiral eventually passes into the margin of the operculum, and for a considerable distance traces it.

If the eye be made to traverse one of the curved lines first spoken of in the operculum of the Turbo, or one of the straight lines in the Nerita, from its margin to the point where it loses itself in the spiral, and if it then follow the spiral until it returns to the point in the margin whence it set out, it will have traversed the boundary of a figure which was once the actual boundary of the operculum, which therefore indicates one stage of its growth, and of which all, similarly traced, will be seen to have similar geometrical forms.

It will further be apparent from this examination, that the operculum has increased at each stage of its growth, not throughout its whole margin at once, but on a series of different portions of it lying in different consecutive positions round it; each such addition being so made as to preserve the above-mentioned geometrical similarity of the whole*. In all the similar geometrical figures thus visible upon the face of the operculum, and which have in succession constituted its limits, the pole of the spiral will moreover be seen to occupy a similar position. The linear dimensions of any two of them ($P_1\ C\ Q_1$ and $P_2\ C\ Q_2$) are then to one another as the radii vectores drawn to similar points in them, and therefore as those ($P\ P_1$ and $P\ P_2$) drawn to the extremities of the boundary by which they unite.

To determine, therefore, the law according to which the linear increase of the operculum takes place, that is, the law according to which the linear increase of the section of the chamber of the shell takes place, we have only to determine the law according to which the radii vectores, drawn to successive points of the spiral visible upon the operculum, increase, that is, we have only, geometrically, to determine the spiral.

Now in every case this spiral is the logarithmic spiral.

A slight inspection of it is sufficient to suggest the probability that the angle at which it intersects its radius vector is everywhere the same, and this supposition is fully confirmed by direct admeasurements grounded upon the following property of the logarithmic spiral, "That the distances of successive spires, measured upon the same radius vector produced, from the pole and from one another, are respectively in geometrical progression; the common ratio of the progression being in both cases $\epsilon^{2\pi \cot A}$, where A is the constant angle of the spiral†."

* The whole class of shells Haliotis affects the method of formation here described. The *shell* itself is in this class generated by additions upon one margin, as in other classes the *operculum* is generated.

† Let R_n, R_{n+1}, R_{n+2} be consecutive radii vectores taken as above, and R_0 the radius vector corresponding to $\theta = 0$ $\therefore R_n = R_0\, \epsilon^{\theta \cot A}$, $R_{n+1} = R_0\, \epsilon^{(\theta + 2\pi) \cot A}$, $R_{n+2} = R_0\, \epsilon^{(\theta + 4\pi) \cot A}$ $\therefore R_{n+1} = \epsilon^{2\pi \cot A}$. R_n and $R_{n+2} - R_{n+1} = \epsilon^{2\pi \cot A}(R_{n+1} - R_n)$.

The following distances were measured upon three different opercula from the poles of their spiral curves to their successive whorls; the distances in the same column being measured on the same radius vector produced. It will be perceived that for the same operculum these distances have the same ratio consecutively to one another; the deviation from this law in no case exceeding that error which of necessity attaches to the method of admeasurement.

Operculum of the Order Turbo, No. I.

Distance in inches.	Ratio.	Distance in inches.	Ratio.	Distance in inches.	Ratio.	Distance in inches.	Ratio.
·24 ·55 1·28	2·28 2·32	·16 ·37 ·85	2·31 2·3	·2 ·6 1·38	2·3 2·3	·18 ·42 ·94	2·3 2·24

Operculum, No. II.

Distance in inches.	Ratio.	Distance in inches.	Ratio.	Distance in inches.	Ratio.
·32 1·25	3·9	·25 1·04	4	·155 ·62	3·87

Operculum, No. III.

Distance in inches.	Ratio.	Distance in inches.	Ratio.
·6 ·8 ·99 1·27	1·33 1·23 1·28	·74 ·95 1·215	1·28 1·28

The spiral of the operculum is then a logarithmic spiral. Now its linear dimensions in the different successive stages of its progress have been shown to be as the successive radii vectores of its spiral. The increments of its linear dimensions are then as the increments of these radii vectores. But by a fundamental property of the logarithmic spiral, the increments of its radii vectores, corresponding to equal increments in their angles of revolution, are as the radii vectores themselves. Thus, then, it follows that the increments of the linear dimensions of the operculum, corresponding to equal angular distances round its pole, are as its existing linear dimensions; and, therefore, that the increments of the linear dimensions of the section of the spiral chamber corresponding to these are everywhere as its existing linear dimensions.

The animal, as he advances in the construction of his shell, increases continually his operculum, so as to adjust it to its mouth.

He increases it, however, not by additions made at the same time all round its

margin, but by additions made only on one side of it at once. One edge of the operculum thus remains unaltered as it is advanced into each new position, and placed in a newly formed section of the chamber similar to the last, but greater than it.

That the same edge which fitted a portion of the first less section should be capable of adjustment, so as to fit a portion of the next similar but greater section, supposes a geometrical provision in the curved form of the chamber of great apparent complication and difficulty. But God hath bestowed upon this humble architect the practical skill of a learned geometrician, and he makes this provision with admirable precision in that curvature of the logarithmic spiral which he gives to the section of the shell. This curvature obtaining, he has only to turn his operculum slightly round in its own plane as he advances it into each newly formed portion of his chamber, to adapt one margin of it to a new and larger surface and a different curvature, leaving the space to be filled up by increasing the operculum wholly on the other margin.

To make this apparent, let the following be received as a characteristic property of the logarithmic spiral: "That lines anywhere drawn from its pole, inclined to one another at the same angle, will intercept between them branches of the curve which, however different their linear dimensions, will be geometrically similar to each other." So that if two lines given in position be imagined to be drawn from the pole of such a spiral, parallel to its plane, and the spiral be then imagined to be put in motion in its own plane round its pole, then as its curve revolved under these lines they would intercept portions of it continually increasing, or continually diminishing in dimensions, and continually receding from or approaching the pole, but all geometrically similarly to each other and similarly placed."

Now each new section of the chamber of the shell being similar to the preceding section, but greater than it, if the operculum were thrust forward into this greater section without being turned round in its own plane, any portion of its edge would manifestly present to the corresponding portion of the perimeter of the new section a similar but a less curve, which could not be made to coincide with it. If, however, the operculum be imagined to be turned round in its own plane about its pole in the opposite direction to that in which the spiral increases, the curve presented by it to this portion of the perimeter of the section will continually approach it, increasing its dimensions, but remaining similar to it, so that at length it will coincide with it. And thus one margin of the operculum will be made everywhere to fit itself to the side of the chamber, the coincidence of the other margin remaining to be produced by new matter added to it.

It will be apparent from a simple inspection of the operculum that the animal does thus turn it round in its own plane as he advances it, with what is called a screw motion.

Such is the theory of the growth of the operculum. There is traced in it the application of properties of a geometric curve to a mechanical purpose by Him who metes the dimensions of space and stretches out the forms of matter according to the

rules of a perfect geometry,—properties which, like so many others in nature, may have also their application in art. It instructs us how to shape a tube of a variable section, so that a piston driven along it shall, by one side of its margin, coincide continually with its surface as it advances, provided only the piston be made at the same time continually to revolve in its own plane.

The investigation has now arrived at a point from which the law of the geometrical description of turbinated shells can be enunciated with greater precision. "They are generated by the revolution about a fixed axis (the axis of the shell) of a curve, which continually varies its dimensions according to the law, that each linear increment, corresponding to a given angular increment, shall vary as the existing dimensions of the line of which it is the increment (the law of the description of the logarithmic spiral), and which curve either retains its position upon the axis, or moves along it with a motion of translation in the direction of its length."

This law is readily subjected to verification by admeasurement.

It is clear that, if it obtain, similar linear dimensions measured at similar points of successive whorls, should be in geometric progression. Thus if the generating curve (as in the *Nautilus Pompilius*) revolve about the axis without at the same time sliding along it, and a section be made through the centre of the shell perpendicular to the axis, then will the section be (if this law be true) a spiral curve, whose distances from the axis, measured on the same radius vector, are in geometrical progression, and which is therefore a logarithmic spiral.

In the more general case, in which the generating curve, as in the *Turbo scalaris*, slides forwards upon the axis as it revolves, increasing at the same time its linear dimensions according to the law of the logarithmic spiral, it is clear that the surfaces of the successive whorls would interfere with one another, and that thus the uniformity of the spiral chamber would be destroyed, unless the motion of translation (or the sliding motion) of the curve, by which the space allowed to each whorl upon the axis is determined, were governed by some law corresponding to that which governs the linear dimensions of the whorl; unless, in short, the spaces allowed to the widths of successive whorls upon the axis varied in the same progression as the widths themselves vary. A similar principle applies to the distances of the whorls measured upon the surface of the shell in the same plane passing through the axis. These distances are, in fact, in this case, similar linear dimensions of successive whorls, and are therefore subject, according to the theory, to the law of the logarithmic spiral, and like the distances of successive whorls of that spiral, on the same radius vector, are in geometric progression.

Nautilus Pompilius.

These conclusions were directly verified by the following observations. A shell of the *Nautilus Pompilius* was cut through the middle in a direction perpendicular to its axis, and a tracing was taken of the section of its spiral surface; this tracing is copied in fig. 6.

It was made from the dark line which shows, on the section of the internal whorls of the shell, the line of that pearly surface which the animal deposits as a covering to its completed portion, as it advances in the construction of it. It is important to make this observation, because as it extends one whorl of its shell over another, the animal deposits continually upon the pearly surface of this last a new coating of shell, and thickens it; and it is in the centre of this thickened section that is to be found that section of the pearly surface, of which the edge of the external whorl is a continuation, and from which this tracing was taken.

It will be found that the distance of any two of its whorls measured upon a radius vector is *one-third* that of the two next whorls measured upon the same radius vector. Thus

$a\,b$ is one-third of $b\,c$,
$d\,e$ is one-third of $e\,f$,
$g\,h$ is one-third of $h\,i$,
$l\,k$ is one-third of $l\,m$.

The curve is therefore a *logarithmic spiral.*

Turbo duplicatus.

From the apex of a large specimen of the *Turbo duplicatus* a line was drawn across its whorls, and their widths were measured upon it in succession, beginning from the last but one. The measurements were, as before, made with a fine pair of compasses and a diagonal scale. The sight was assisted by a magnifying glass.

In a parallel column to the admeasurements are the terms of a geometric progression, whose first term is the width of the widest whorl measured, and whose common ratio is 1·1804.

Widths of successive whorls measured in *inches* and parts of an *inch.*	Terms of a geometrical progression, whose first term is the width of the widest whorl, and whose common ratio is 1·1804.
1·31	1·31
1·12	1·1098
·94	·94018
·8	·79651
·67	·67476
·57	·57164
·48	·48427
·41	·41026

Yet further to verify this remarkable coincidence of the widths of successive whorls with the mathematical law of a geometric progression, the following property of such a progression was determined: "that μ representing the ratio of the sum of every even number (m) of its terms to the sum of half that number of terms, the common ratio (r) of the series is represented by the formula

$$r = (\mu - 1)^{\frac{2}{m}}."$$

The following measurements were then made, beginning from the second and third whorls respectively:

Width of six whorls in inches.	Width of three whorls in inches.	Ratio μ.
5·37	2·03	2·645
4·55	1·72	2·645
Width of four whorls in inches.	**Width of two whorls in inches.**	**Ratio μ.**
4·15	1·74	2·385
3·52	1·47	2·394

By the two first admeasurements the formula gives

$$r = (1{\cdot}645)^{\frac{1}{3}} = 1{\cdot}1804.$$

By the mean of the ratios deduced from the two second admeasurements it gives

$$r = (1{\cdot}389)^{\frac{1}{2}} = 1{\cdot}1806.$$

It is scarcely possible to imagine a more accurate verification than is deduced from these larger admeasurements, and we may with safety annex to the species *Turbo duplicatus* the characteristic number 1·18.

Buccinum subulatum.

A line was drawn from the apex of this shell across its whorls as in the last, and the following admeasurements were similarly made.

Widths of successive whorls by admeasurement in inches.	Terms of a geometrical progression, whose first term is the width of the widest whorl and ratio, 1·13.
1·14	1·14
1·00	1·0089
·9	·89279
·79	·79008
·7	·69919
·62	·61875
·54	·54757

In verification of the above the following larger admeasurements were made, beginning respectively from the last whorl, the last but one, and the last but two.

Width of six whorls.	Width of three whorls.	Ratio μ.
5·10	2·08	2·45
4·52	1·84	2·45
3·94	1·60	2·45

From these admeasurements we have, by the formula $r = (\mu - 1)^{\frac{2}{m}}$,

$$r = (1{\cdot}45)^{\frac{1}{3}} = 1{\cdot}1318.$$

Turbo phasianus.

Three lines were drawn from the apex of this shell in different directions across its whorls, and the following admeasurements were made upon them:

Widths of successive whorls in inches by admeasurement.	Terms of a geometrical progression, whose first term is the width of the widest whorl and ratio, 1·75.
First line.	First line.
2·55	2·55
·44	·44
·25	·251
Second line.	Second line,
·98	·98
·56	·56
·32	·323
Third line.	Third line.
·7	·7
·4	·4
·23	·228

The remarkable accordance of the measured with the calculated widths of the whorls in this shell is to be attributed to the precision with which the line of separation of the whorls is traced upon it. A great number of admeasurements were similarly made upon other shells of the genera Trochus, Strombus and Murex; some of them were cut through the axis longitudinally; and similar measurements were made by drawing lines from the apex across the section. From all these the same result was obtained.

Thus to each particular species of shell is annexed a characteristic number, being the ratio of the geometric progression of similar successive linear dimensions of its whorls; from which number is deducible the constant angle of the particular logarithmic spiral which is affected by that species of shell (see equation 26. in the following mathematical discussion). This number, or this angle, connected as it is of necessity with the circumstances of the animal's growth and the manner of its existence, is determinable by actual admeasurement, and may be available for the purposes of classification; it may suggest relations to which the observations of naturalists may usefully be directed, and eventually become linked with characteristic forms and modes of molluscous existence*.

* The whole race of land shells, for instance, will certainly (from the nature of the case) be distinguished from the aquatic shells by a wide difference in the numbers characteristic of the species of the two groups.

Why the Mollusks who inhabit turbinated and discoid shells should, in the progressive increase of their spiral dwellings, affect the particular law of the logarithmic spiral, is easily to be understood. Providence has subjected the instinct which shapes out each, to a rigid *uniformity* of operation.

This uniformity manifests itself in turbinated shells in respect to their axes. Now the law of the logarithmic spiral, considered under its more general form of a curve of double curvature, is the only one according to which the Mollusk can wind its spiral dwelling in an uniform direction through the space round its axis, in respect to that axis. Under this general form it may be geometrically defined as the curve whose tangent retains always the same angular position in respect to its axis*, and in respect to a line drawn from the point where it touches the curve perpendicular to the axis; or in other words, which traverses the space round the axis always in the same direction in respect to it.

A second property of the logarithmic spiral, equally referring itself to the uniformity of the animal's operations about the axis of its shell, is this; that it has everywhere the same geometrical curvature, and is the only curve except the circle† which possesses this property.

Certain physiological facts having reference to the growth of the Mollusk are deducible from the geometrical description of its shell. If it be a *land* shell, its capacity may be supposed (reasoning from that principle of economy which is an observable law in Nature) to be precisely sufficient for the reception of the animal who built it. If it be an *aquatic* shell, it serves the animal at once as a habitation and as a float; enabling it to vary its buoyancy according as it leaves a greater or a less portion of the narrower extremity of its chamber unoccupied, and thus to ascend or descend in the water, at will. Now that its buoyancy, and therefore the facility of thus varying its position, may remain the same at every period of its growth, it is necessary that the increment of the capacity of its float should bear a constant ratio to the corresponding increment of its body, a ratio which always assigns a greater amount to the increment of the capacity of the shell than to the corresponding increments of the animal's bulk. Thus the chamber of the *aquatic* shell is increased, not only, as is the land shell, so that it may contain the greater bulk of the Mollusk, but so that more and more of it may be left unoccupied. Now the capacity of the shell and the dimensions of the animal began together, and they increase thus in a constant ratio; the whole bulk of the animal bears therefore a constant ratio, of greater inequality, to the whole capacity of the shell, in *aquatic* shells: in *land* shells, it is probably equal to it.

Now let the generating curve of a shell be conceived to describe, as it revolves round its axis, a series of successive equal angles, represented each by $\Delta\,\Theta$. Corresponding to these equal increments of the angle of revolution of the generating

* So that moved parallel to itself until it intersected the axis, it would always intersect it at the same angle.

† The circle may, in fact, be considered a logarithmic spiral, the constant inclination of whose tangent to its radius vector is a right angle. Of all curves, this spiral, considered as thus including the circle, is the simplest.

curve, will be certain increments of the capacity of the shell; and it appears from the following mathematical investigation of the properties of conchoidal surfaces (see equation 19.), that the increments of the capacity of the shell, thus taken, will be in a constant ratio to the then existing whole capacities of the shell. The increment of the animal's bulk corresponding to each of these increments of the shell must then be in a constant ratio to its then existing bulk; that is, the animal's growth corresponding to a given increment, $\Delta\,\Theta$, in the angle described by the generating curve of its shell, is proportional always to its existing growth.

Let us now suppose that the physical living energies of the animal (those by which it grows), at any time, are proportional to its then existing growth; and therefore that its growth in any increment of time is proportional to its growth up to that time (a supposition which possesses an independent probability). From the conclusion before arrived at, and from this supposition, it follows that the growth of the animal corresponding to a given increment, $\Delta\,\Theta$, in the angle of revolution of the generating curve, and the growth corresponding to a given increment of time, are each proportional to the animal's whole then existing growth, and therefore to one another; and, since they begin together, that the whole angle, Θ, of revolution of the generating curve of the shell, is proportional to the whole corresponding time of the animal's growth, and therefore that the whole number of whorls, and parts of whorls, is proportional to its whole age: a conclusion which, like the supposition whence it is deduced, possesses an independent probability.

The separate probability of each of the two suppositions, "that the physical energies of the Mollusk, as developed in its growth in a given increment of time, are proportional to its whole then existing growth*, and that its age is always proportional to the whole angle which, in the construction of the shell, it has then described round its axis," is greatly increased by the necessary relation which is here shown to obtain between them; a relation, by reason of which, either supposition being made, the other becomes a conclusion.

The form of the Mollusk being supposed to remain geometrically similar to itself, the surface of its mantle, by which organ it deposits its shell, of necessity varies as the square of its linear dimensions, whilst the whole bulk of the animal varies as the cube of its linear dimensions. But, as its whole bulk, varies its active living and growing energy, and therefore the amount of the deposition of its shell in a given time; this last, then, varies as the cube of its linear dimensions; but the surface of the depositing organ (the mantle) varies only as the square of the same linear dimensions. Besides, then, the organic increase of the surface of the mantle, there must be an increased functional activity of all its organs, varying as its simple linear dimensions.

This increased functional activity of the surface of the depositing organ, varying

* May not this law of the growth of a Mollusk have its analogy in other forms of animal life, and perhaps in vegetable life?

simply as the linear dimensions of the animal or its shell, offers an analogy, and has perhaps a relation, to the increase of the section of the shell, according to the same law of its simple linear dimensions.

Subjoined to this paper is a mathematical discussion of the following geometrical and mechanical elements of a conchoidal surface: its VOLUME, the dimensions of its SURFACE, the CENTRE OF GRAVITY of its contained solid, the CENTRE OF GRAVITY of its surface.

These elements are determined (the law of the logarithmic spiral being supposed) by certain transcendental functions, having constant factors dependent for their amount upon the statical moments and the moments of inertia of the generating figures and of their areas.

The object proposed in the determination of these elements was their application to a discussion of the hydraulic theory of shells; yet further, if possible, to develope that wisdom of God which shaped them out and moulded them; and especially in reference to the particular value of the constant angle which the spiral of each species of shell affects,—a value connected by a necessary relation with the economy of the material of each, and with its stability, and the conditions of its buoyancy*.

The paper concludes with a discussion of the general EQUATIONS to a conchoidal surface in respect to systems of polar and of rectangular coordinates.

To determine the Volume of a Conchoidal Solid.

Suppose the generating curve to be a plane curve, and let it (first) retain its position upon the axis as it revolves, varying its dimensions.

Let P C and Q C (fig. 3.) be two of its positions, inclined at the angle $\Delta\,\Theta$, and including between them the elementary solid P C Q.

Imagine the plane P C to have revolved about A z through the angle $\Delta\,\Theta$ without altering its dimensions, the solid generated by it would then, by the theorem of GULDINUS, be represented by $M\,.\,\Delta\,\Theta$, where M represents the statical moment of the plane P C about the axis A z.

The elementary solid imagined to be in like manner generated by the revolution of Q C through the angle $\Delta\,\Theta$, will similarly be represented by $(M+\Delta\,M)\,\Delta\,\Theta$.

Now between these two imaginary solids is evidently the actual elementary solid P C Q. Calling then V the volume of the solid to be determined, we have

$$M\,\Delta\,\Theta < \Delta\,V < (M+\Delta\,M)\,\Delta\,\Theta.$$

Or, considering M and V as functions of Θ, and expanding by TAYLOR's theorem,

$$M\,\Delta\,\Theta < \frac{d\,V}{d\,\Theta}\,\Delta\,\Theta + \frac{d^2\,V}{d\,\Theta^2}\cdot\frac{(\Delta\,\Theta)^2}{1\,.\,2} + \&c. < M\,\Delta\,\Theta + \frac{d\,M}{d\,\Theta}\,(\Delta\,\Theta)^2 + \&c.$$

* As illustrative of this remark, it may here be mentioned that the shell of the *Nautilus Pompilius* has, hydrostatically, an *A*-statical surface. If placed with any portion of its surface upon the water, it will immediately turn over towards its smaller end, and rest only on its mouth. Those conversant with the theory of floating bodies will recognise in this an interesting property.

And this is true for all values of Θ:

$$\therefore \frac{d\,V}{d\,\Theta} = M,$$

and

$$V = \int M\,d\,\Theta. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1.)$$

If we imagine the plane C Q to slide along the axis A z (fig. 4.) without otherwise altering its position, the elementary solid included between it and P C will retain the same volume as it had before; for the two planes P C and Q C may be divided into the same number of similar elements, whose corresponding angles being joined, the solid element included between them will be divided into as many pyramidal frusta, the volume of each of which will remain unaltered by the supposed displacement of C Q, since each such frustum may be imagined to be made up of two pyramids, the base of each of which will remain the same after the displacement, and their bases and vertices between the same parallels. Thus, then, the volume determined by the above formula is that of the conchoidal solid under its most general form.

To determine the Area of a Conchoidal Surface.

Let U represent the whole area of the surface (fig. 3.), and Δ U the elementary area intercepted between the positions P C and Q C of the generating curve, supposed to revolve without otherwise altering its position on the axis.

Take Δ S to represent the element P Q of the curve described by the extremity P of the revolving axis P C of the generating curve.

Imagine the generating curve to describe, without altering its dimensions, an angle about the axis A z, such that the circular arc, described on this supposition by the point P, may equal P Q or Δ S. This angle will be represented by $\frac{\Delta\,S}{R}$.

The generating curve remaining always similar to itself, its statical moment about A z is a function of Θ or of R. Let it be represented by N, and considered a function of R. The elementary surface which the curve C P will generate, on the supposition just made, will then be represented by $\frac{\Delta\,S}{R} \cdot N$, according to the property of Guldinus.

A surface similarly generated by C Q will in like manner be represented by

$$\frac{\Delta\,S}{R + \Delta\,R}\,(N + \Delta\,N).$$

Now the dimensions of the actual element of the conchoidal surface lie between the dimensions of these two imaginary surfaces.

This will be seen if we conceive any number of planes passing through the axis C D, at right angles to A z, to intersect all three of the surfaces spoken of. The intercepted parts will be strips of the three surfaces, all of the same length, but of

breadths, of which those of the surface described by P C will be the least, and those described by Q C the greatest.

$$\therefore \frac{N}{R} \Delta S < \Delta U < \frac{N + \Delta N}{R + \Delta R} \cdot \Delta S.$$

Considering, therefore, S, U, and N as functions of R, expanding by TAYLOR's theorem and dividing by Δ R

$$\frac{N}{R} \cdot \frac{dS}{dR} + \frac{d^2S}{dR^2} \cdot \frac{N}{R} \cdot \frac{\Delta R}{1.2} + \&c. < \frac{dU}{dR} + \frac{d^2U}{dR^2} \cdot \frac{\Delta R}{1.2} + \&c. < \frac{N}{R} \cdot \frac{dS}{dR}$$

$$+ \left\{ \frac{dS}{dR} \left(\frac{1}{N} \frac{dN}{dR} - \frac{1}{R} \right) + \frac{1}{2} \frac{d^2S}{dR^2} \right\} \Delta R + \&c.$$

The second of these series having, for all values of Δ R, a value intermediate between the other two, and the first terms of these other two being equal; the first terms of the three series are equal.

$$\therefore \frac{dU}{dR} = \frac{N}{R} \cdot \frac{dS}{dR}$$

and

$$U = \int \frac{N}{R} \cdot \frac{dS}{dR} \cdot dR \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

which to adapt it for integration, (R being a function of Θ) may be put under the form

$$U = \int \frac{N}{R} \cdot \frac{dS}{dR} \cdot \frac{dR}{d\Theta} \cdot d\Theta.$$

The expression for the area of the surface thus determined, on the supposition that the generating curve does not alter its position in respect to the axis otherwise than by revolving round it, is the same with that of the surface which would be generated by a curve which, as it revolved about the axis, slided along it, a different form being assigned to the function N. For if we imagine a conchoidal surface of this general form (fig. 4.) to be intersected by planes, exceedingly near to one another, passing through its axis, and at the same time to be traversed, as the surfaces of turbinated shells usually are, by spiral lines parallel to the direction of the whorl, and which may be understood to mark the paths of given points in the generating curve*; then each element of the surface intercepted between two of the planes spoken of will, by these spiral lines, be divided into a series of oblique parallelograms, two adjacent sides (containing an acute angle) of each of which, may be considered as intersections with the conchoidal surface of two planes, which intersect one another in an ordinate of the generating curve; one of these planes is a tangent to one of the spiral lines spoken of, and the other is the plane of the generating curve itself. Now let us suppose the inclination of these planes to one another to be *constant*, as is always the case in shells, and let it be represented by A. Let moreover the inclination, to its ordinate,

* This demonstration will be best understood by referring to the actual surface of a turbinated shell on which the spiral lines are visible.

of the tangent to the generating curve be represented by φ; and the inclination to the same ordinate of the tangent to the spiral line by σ. We have then given the inclination A of two planes to one another, and the inclinations φ and σ of two lines, drawn in them respectively, to the intersection of these planes; whence by a well-known formula of spherical trigonometry, if ι represent the inclination of these lines to one another,

$$\cos \iota = \cos \varphi \cos \sigma + \sin \varphi \sin \sigma \cos A.$$

Moreover, if the two adjacent sides of the parallelogram, being elements of the generating curve, and the spiral, be represented by Δs and ΔS; then since their inclination is ι, the area of the parallelogram is represented by $\Delta S . \Delta s \sin \iota$. Now let us suppose the generating curve to revolve, not altering its dimensions, but sliding along the axis; then

$$\sigma = \frac{\pi}{2} \therefore \cos \iota = \sin \varphi \cos A, \text{ and } \sin \iota = \sqrt{1 + \sin^2 A \tan^2 \varphi} . \cos \varphi;$$

also in this case

$$\Delta S = y \Delta \Theta \operatorname{cosec} A;$$

the area of the elementary parallelogram becomes then

$$y \sqrt{\operatorname{cosec}^2 A + \tan^2 \varphi} . \cos \varphi . \Delta s . \Delta \Theta, \quad \text{or } y \sqrt{\operatorname{cosec}^2 A + \frac{d x^2}{d y^2}} \cdot \frac{d y}{d s} \Delta s \Delta \Theta;$$

so that the whole surface of the elementary slice intercepted between two planes passing through the axis which are inclined to one another at an angle $\Delta \Theta$, is on this supposition,

$$\Delta \Theta \int y \sqrt{\operatorname{cosec}^2 A + \frac{d x^2}{d y^2}} \cdot d y.$$

Suppose the integral in this expression to be represented by N^1, then N^1 will become N (as it ought) in that particular case in which, the curve not sliding along the axis, A becomes $\frac{\pi}{2}$.

Now we may reason in respect to N^1 precisely as before in respect to N, and we shall obtain, by the same steps, the same expression for the surface in terms of N^1, as was then obtained in terms of N.

To find the Centre of Gravity of a Conchoidal Solid.

Suppose the solid included between P C and Q C (fig. 3.) to be divided into an infinite number of prismatic elements by planes perpendicular to P C, and perpendicular and parallel to A z; and let $m r$ (fig. 5.) represent one of these elements.

The VOLUME of this element is represented by

$$\overline{n o} \times \overline{n s}$$

or by

$$\frac{1}{2} (m n + o p) \overline{n p} . \overline{n s}$$

or by

$$\frac{1}{2}(m\,n + o\,p)\,\overline{n\,r}$$

or by

$$\frac{1}{2}(u\,m + u\,o)\,\overline{.\,m\,q}\,.\sin\Delta\,\Theta\,.\cos\Delta\,\Theta\,. \quad \ldots \ldots \quad (3.)$$

The MOMENTUM of the element about a plane passing through A z, and perpendicular to P C, (fig. 3.) is therefore represented by

$$\frac{1}{4}(u\,m + u\,o)^2\,\overline{m\,q}\,.\sin\Delta\,\Theta\,.\cos^2\Delta\,\Theta$$

or by

(the momentum of inertia of the plane $m\,q$) $\sin\Delta\,\Theta\,.\cos^2\Delta\,\Theta$.

Assuming then Θ to be measured from the plane $z\,y$, the momentum of the element $m\,r$ about the plane $z\,y$ is represented by

(momentum of inertia of elementary plane $m\,q$) $\sin\Theta\sin\Delta\,\Theta\cos^2\Delta\,\Theta$,

and the momentum of the same element about the plane $z\,x$ is represented by

(momentum of inertia of elementary plane $m\,q$) $\cos\Theta\sin\Delta\,\Theta\cos^2\Delta\,\Theta$.

Hence if we imagine two solids to be generated, one by the revolution of P C, without altering its dimensions, through the angle P C Q, and the other by the revolution of Q C through the same angle; and if we take I to represent the momentum of inertia of the plane P C; then will the momentum of the first solid about the plane $z\,y$, be represented by

$$\mathrm{I}\sin\Theta\sin\Delta\,\Theta\cos^2\Delta\,\Theta,$$

and that of the second by

$$\left(\mathrm{I} + \frac{d\,\mathrm{I}}{d\,\Theta}\Delta\,\Theta + \ldots.\right)\sin(\Theta + \Delta\,\Theta)\sin\Delta\,\Theta\cos^2\Delta\,\Theta.$$

Now the momentum of the elementary solid P C Q evidently lies between those of these elementary solids. Calling then the momentum of the whole solid, of which P C Q is an element, M_1, when estimated in respect to the plane $z\,y$, we have

$$\mathrm{I}\sin\Theta\cos^2\Delta\,\Theta\sin\Delta\,\Theta < \frac{d\,\mathrm{M}_1}{d\,\Theta}\Delta\,\Theta + \&c.\ldots < \mathrm{I}\sin\Theta\cos^2\Delta\,\Theta\sin\Delta\,\Theta + \&c.$$

$$\therefore \mathrm{I}\sin\Theta\cos^2\Delta\,\Theta < \frac{d\,\mathrm{M}_1}{d\,\Theta}\cdot\frac{\Delta\,\Theta}{\sin\Delta\,\Theta} + \&c.\ldots < \mathrm{I}\sin\Theta\cos^2\Delta\,\Theta + \&c.$$

And this is true for all values of $\Delta\,\Theta$.

$$\therefore \frac{d\,\mathrm{M}_1}{d\,\Theta} = \mathrm{I}\sin\Theta.$$

Similarly calling M_2 the moment of the whole solid about the plane $z\,x$

$$\frac{d\,\mathrm{M}_2}{d\,\Theta} = \mathrm{I}\cos\Theta$$

$$\therefore \mathrm{M}_1 = \int \mathrm{I}\sin\Theta\,d\,\Theta$$

$$M_2 = \int I \cos \Theta \, d \Theta$$

$\therefore$ distance of centre of gravity from plane $z\,y = \dfrac{\int I \sin \Theta \, d\Theta}{\int M \, d\Theta}$, . . . (4.)

$\therefore$ distance of centre of gravity from plane $z\,x = \dfrac{\int I \cos \Theta \, d\Theta}{\int M \, d\Theta}$. . . . (5.)

The generating curve has here been supposed to revolve about the axis A z, otherwise retaining its position upon it.

If we suppose P C to slide along the axis as it revolves (fig. 4.), the moment of the elementary solid P C Q about A z, and therefore the moments M_1 and M_2 of the whole solid about the planes $z\,x$ and $z\,y$ will remain unaltered.

Another dimension will however now have become necessary to determine the position of the centre of gravity; viz. its distance from a given point in the axis A z, measured along that axis.

Let V (fig. 4.) be the point where the generating curve intersects the axis A z; $\therefore$ by equation (3.) the momentum of the element $m\,r$ (fig. 5.) about a plane passing through V perpendicular to the axis A z is represented by

$$\frac{1}{2} V u \, (u\,m + u\,o) \, \overline{m\,q} \, . \sin \Delta \Theta \, . \cos \Delta \Theta \, ;$$

and assuming V $u = x$, and $u\,m$ (figs. 4 and 5.) $= y$, the momentum of the whole elementary solid generated by the revolution of P C through the angle $\Delta \Theta$ is represented by

$$\int \int x\,y \, d\,x \, d\,y \, . \sin \Delta \Theta \, . \cos \Delta \Theta.$$

And representing $\int\int x\,y\,d\,x\,d\,y$ by L, and reasoning as before, the moment of the whole solid about a plane perpendicular to A z passing through V is represented by

$$\int L \, d\Theta.$$

And if A V $= z$, the distance of the centre of gravity from A measured along the axis is represented by

$$z + \frac{\int L \, d\Theta}{\int M \, d\Theta}. \quad . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \; . \quad (6.)$$

To find the Centre of Gravity of a Conchoidal Surface.

Imagine the generating curve to describe, without altering its dimensions, an angle about the axis A z (fig. 3.), such that the circular arc described on this supposition by the point P may equal the element P Q of the length of the curve or Δ S; this angle will be represented by $\frac{\Delta S}{R}$. Moreover, the moment of the elementary surface thus generated about the plane $z\,y$ will be represented by

$$\int y^2 \sin \Theta \frac{\Delta S}{R} \, d\,s,$$

where y is any ordinate of the generating curve at right angles to the axis A z, and s is taken to represent the length of the generating curve. Assuming then $\int y^2 \, d s$ or the moment of inertia of the perimeter of the curve to be represented by K, the moment of this imaginary surface about the plane $z \, y$ is represented by

$$\frac{\mathrm{K}}{\mathrm{R}} \sin \Theta \, \Delta \, \mathrm{S},$$

and, similarly, that about the plane $z \, x$ is represented by

$$\frac{\mathrm{K}}{\mathrm{R}} \cos \Theta \, \Delta \, \mathrm{S}.$$

Conceiving now a similar elementary surface to be generated by the curve Q C without changing its dimensions, the two moments of that surface will be represented by

$$\frac{\mathrm{K} + \Delta \, \mathrm{K}}{\mathrm{R} + \Delta \, \mathrm{R}} \cos \left(\Theta + \Delta \, \Theta\right) \Delta \, \mathrm{S}$$

and

$$\frac{\mathrm{K} + \Delta \, \mathrm{K}}{\mathrm{R} + \Delta \, \mathrm{R}} \sin \left(\Theta + \Delta \, \Theta\right) \Delta \, \mathrm{S}.$$

Moreover, the moment of the actual element of the conchoidal surface evidently lies between the moments of these imaginary elements; as before, therefore, the whole moments of the conchoidal surface about the planes $z \, x$ and $z \, y$, being represented by N_1 and N_2,

$$\frac{d \, \mathrm{N}_1}{d \Theta} = \frac{\mathrm{K}}{\mathrm{R}} \sin \Theta \frac{d \, \mathrm{S}}{d \Theta}$$

$$\frac{d \, \mathrm{N}_2}{d \Theta} = \frac{\mathrm{K}}{\mathrm{R}} \cos \Theta \frac{d \, \mathrm{S}}{d \Theta}.$$

Similarly, if N_3 represent the moment of the surface about a vertical plane perpendicular to the axis A z, and passing through the point V; and if x be an abscissa to any point of the generating curve measured along the axis from that point; and if H represent the integral $\int x \, y \, d \, s$, taken in respect to the whole perimeter of the generating curve; then

$$\frac{d \, \mathrm{N}_3}{d \Theta} = \frac{\mathrm{H}}{\mathrm{R}} \cdot \frac{d \, \mathrm{S}}{d \Theta}.$$

The distances of the centre of gravity from the planes $z \, y$, $z \, x$, and $x \, y$, are then respectively

$$\frac{\int \frac{\mathrm{K}}{\mathrm{R}} \sin \Theta \, d \, \mathrm{S}}{\int \frac{\mathrm{N}}{\mathrm{R}} d \, \mathrm{S}} \quad \ldots \ldots \ldots \ldots \quad (7.)$$

$$\frac{\int \frac{\mathrm{K}}{\mathrm{R}} \cos \Theta \, d \, \mathrm{S}}{\int \frac{\mathrm{N}}{\mathrm{R}} d \, \mathrm{S}} \quad \ldots \ldots \ldots \ldots \quad (8.)$$

and

$$z + \frac{\int \frac{\mathrm{H}}{\mathrm{R}} d \, \mathrm{S}}{\int \frac{\mathrm{N}}{\mathrm{R}} d \, \mathrm{S}} \quad \ldots \ldots \ldots \ldots \quad (9.)$$

To determine the Area of the Surface and the Centre of Gravity of a Turbinated Shell, and the Volume and Centre of Gravity of its Contained Solid.

The generating curve of a turbinated shell remains similar to itself as it revolves; the statical moment of its perimeter varies therefore as the square, and the moment of inertia of its perimeter as the cube, of any of its linear dimensions. In like manner the statical moment of the area of its generating curve varies as the cube, and the moment of inertia as the fourth power, of any of its linear dimensions.

If therefore C_1 C_2 C_3 C_4 C_5 C_6 represent certain constants determined by the geometrical conditions of the generating curve,

$$N = C_1 R^2 \quad K = C_2 R^3 \quad H = C_3 R^3$$
$$M = C_4 R^3 \quad I = C_5 R^4 \quad L = C_6 R^4.$$

Therefore the surface of the shell is represented by the integral

$$C_1 \int R \, d S. \qquad (10.)$$

The co-ordinates of the centre of gravity of the surface are represented by

$$\left.\begin{array}{c} \dfrac{C_2 \int R^2 \sin \Theta \, d S}{C_1 \int R \, d S}, \quad \dfrac{C_2 \int R^2 \cos \Theta \, d S}{C_2 \int R \, d S} \\ \text{and} \\ z + \dfrac{C_3 \int R^2 \, d S}{C_1 \int R \, d S}. \end{array}\right\} \qquad (11.)$$

And the volume of the contained solid is represented by

$$C_4 \int R^3 \, d \Theta. \qquad (12.)$$

The co-ordinates of the centre of gravity of the contained solid are represented by

$$\left.\begin{array}{c} \dfrac{C_5 \int R^4 \sin \Theta \, d \Theta}{C_4 \int R^3 \, d \Theta}, \quad \dfrac{C_5 \int R^4 \cos \Theta \, d \Theta}{C_4 \int R^3 \, d \Theta} \\ z + \dfrac{C_6 \int R^4 \, d \Theta}{C_4 \int R^3 \, d \Theta}. \end{array}\right\} \qquad (13.)$$

Now it has been shown that in shells R varies according to the law of the logarithmic spiral; so that

$$R = R_0 \, \varepsilon^{\Theta \cot A},$$

where R_0 is the value of R when $\Theta = 0$, and A is the constant angle which the radius vector of the spiral makes with its tangent, whether it be a plane curve or a curve of double curvature; whence it may readily be proved that

$$\frac{d S}{d \Theta} = R \operatorname{cosec} A. \qquad (14.)$$

Hence, substituting in the preceding formula, and integrating by the known rules, we obtain, for the surface of the shell, the expression

$$\frac{1}{2} C_1 R_0^2 \sec A \left(\varepsilon^{2 \Theta \cot A} - 1\right); \qquad (15.)$$

for the co-ordinates of the centre of gravity,

$$\frac{2\,C_2\,R_0}{C_1\,(\tan A + 9\cot A)} \cdot \frac{(3\cot A\sin\Theta - \cos\Theta)\,\varepsilon^{3\,\Theta\cot A} + 1}{\varepsilon^{2\,\Theta\cot A} - 1} \quad \ldots\ldots \quad (16.)$$

$$\frac{2\,C_2\,R_0}{C_1\,(\tan A + 9\cot A)} \cdot \frac{(3\cot A\cos\Theta + \sin\Theta)\,\varepsilon^{3\,\Theta\cot A} - 3\cot A}{\varepsilon^{2\,\Theta\cot A} - 1} \quad \ldots \quad (17.)$$

$$r_0\left(\varepsilon^{(\Theta - 2n\pi)\cot A} - 1\right) + r_0\,\varepsilon^{(\Theta - 2\pi)\cot A} \cdot \left(\frac{\varepsilon^{-2n\pi\cot A} - 1}{\varepsilon^{-2\pi\cot A} - 1}\right) + \frac{2}{3}\left(\frac{C_3\,R_0}{C_1}\right)\left(\frac{\varepsilon^{3\Theta\cot A} - 1}{\varepsilon^{2\Theta\cot A} - 1}\right). \quad (18.)$$

Observing that r_0 being taken to represent the initial length of the lesser diameter* V T, of the generating curve, $r_0\,\varepsilon^{\Theta\cot A}$ will represent the length of that diameter after the generating curve has revolved through the angle Θ, and $r_0\,\varepsilon^{(\Theta-2\pi)\cot A}$ will represent the width of the next preceding whorl of the shell, measured in the direction of this diameter produced; and the sum of the widths of all the preceding whorls, supposed to be n in number, and measured in this direction, will be represented by

$$\Sigma^n_1\,r_0\,\varepsilon^{(\Theta - 2n\pi)\cot A}.$$

Moreover, that the lesser diameter sliding along the axis, as the curve revolves through any angle, a distance precisely equal to that by which the diameter increases, it follows that the distance from the edge of the last or nth of the preceding whorls, measured in this direction, to the origin is represented by

$$r_0\left(\varepsilon^{(\Theta-2n\pi)\cot A} - 1\right).$$

So that z_1† is represented by the formula

$$r_0\left(\varepsilon^{(\Theta - 2n\pi)\cot A} - 1\right) + \Sigma^n_1\,r_0\,\varepsilon^{(\Theta - 2n\pi)\cot A}.$$

Integrating the formula (12.), having substituted for the value of R, we find for the VOLUME of the solid contained by the shell the expression

$$\frac{1}{3}\,C_4\,R_0^3\tan A\left(\varepsilon^{3\,\Theta\cot A} - 1\right).\ddagger \quad \ldots\ldots\ldots\ldots \quad (19.)$$

And integrating the formula (13.), the co-ordinates of the centre of gravity of the contained solid are found to be

$$\frac{3\,C_5\,R_0}{C_4\,(\tan A + 16\cot A)} \cdot \frac{(4\cot A\sin\Theta - \cos\Theta)\,\varepsilon^{4\,\Theta\cot A} + 1}{\varepsilon^{3\,\Theta\cot A} - 1} \quad \ldots\ldots \quad (20.)$$

$$\frac{3\,C_5\,R_0}{C_4\,(\tan A + 16\cot A)} \cdot \frac{(4\cot A\cos\Theta + \sin\Theta)\,\varepsilon^{4\,\Theta\cot A} - 4\cot A}{\varepsilon^{3\,\Theta\cot A} - 1} \quad \ldots \quad (21.)$$

* When the whorls partially overlap one another, this diameter is to be understood to extend only across that portion of the generating curve which actually generates the chamber of the shell, and which is not interfered with by the preceding whorl. In these cases, then, it will only be a portion of what would be the shorter diameter of the generating curve, if that curve were completed.

† In the case in which the generating curve does not slide upon the axis as it revolves, $z_1 = 0$.

‡ In the case of turbinated shells R_0 may be considered extremely small with respect to any existing dimensions, and Θ exceedingly great, so that the formula 19. being taken to represent the whole capacity of the shell, becomes in this case $\frac{1}{3}\,C_4\,R_0^3\,\varepsilon^{3\,\Theta\cot A}$, and varies as R^3.

$$r_0\left(\varepsilon^{(\Theta-2n\pi)\cot A}-1\right)+r_0\,\varepsilon^{(\Theta-2\pi)\cot A}\cdot\left(\frac{\varepsilon^{-2n\pi\cot A}-1}{\varepsilon^{-2\pi}-1}\right)+\frac{3}{4}\left(\frac{C_6 R_0}{C_4}\right)\left(\varepsilon^{4\Theta\cot A}-1.\right) \quad (22.)$$

To determine the Polar Equation to the surface of a Turbinated Shell.

Let m (fig. 4.) be any point in the surface of the shell, and let the equation to the curve V Q T, imagined to be in the act of generating the point m, be

$$y_1=f(B, x_1)$$

where B is an arbitrary constant representing a linear dimension of the curve, and therefore varying according to the law of the logarithmic spiral, so that it may be represented by the formula

$$B=B_0\,\varepsilon^{\Theta\cot A},$$

where B_0 is the initial value of B. Suppose the abscissæ of the curve to be measured along the axis V T from V, so that V u and $u\,m$ are co-ordinates of m. Let A $m=\varrho$, m A $z=\Phi$, A $u=\varrho\cos\Phi=z_1+x_1$, $u\,m=\varrho\sin\Phi=y_1$

$$\therefore \varrho\sin\Phi=f\left(B_0\,\varepsilon^{\Theta\cot A},\ \varrho\cos\Phi-z_1\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23.)$$

or substituting for z_1 its value

$$\varrho\sin\Phi=f\left\{B_0\,\varepsilon^{\Theta\cot A},\varrho\cos\Phi-r_0\left(\varepsilon^{(\Theta-2n\pi)\cot A}-1\right)-r_0\,\varepsilon^{(\Theta-2n\pi)\cot A}\left(\frac{\varepsilon^{-2n\pi\cot A}-1}{\varepsilon^{-2\pi\cot A}-1}\right)\right\} \quad (24.)$$

From the above may readily be determined the equation to the surface of a shell between the rectangular co-ordinates x, y, z. Observing that $\Theta-2\,n\,\pi=\tan^{-1}\frac{x}{y}$, and substituting, we obtain

$$\left.\begin{array}{l}(x^2+y^2)^{\frac{1}{2}}\\ =f\left\{B_0\,\varepsilon^{\left(2n\pi+\tan^{-1}\frac{x}{y}\right)\cot A},z-r_0\left(\varepsilon^{\tan^{-1}\frac{x}{y}\cot A}-1\right)-r_0\,\varepsilon^{\tan^{-1}\frac{x}{y}\cot A}\cdot\left(\frac{\varepsilon^{-2n\pi\cot A}-1}{\varepsilon^{-2\pi\cot A}-1}\right)\right\}\end{array}\right\}(25.)$$

The values of the constants C_1 C_2 C_3 C_4 C_5 C_6 are dependent upon the geometrical form of the generating curve in each particular shell; the constants R_0 r_0 and B_0 on its dimensions at the point where the generation of the shell is supposed to commence.

The constant A is independent of the form and dimensions of the generating curve. It depends simply upon the law of that particular logarithmic spiral which is affected by that species of shell.

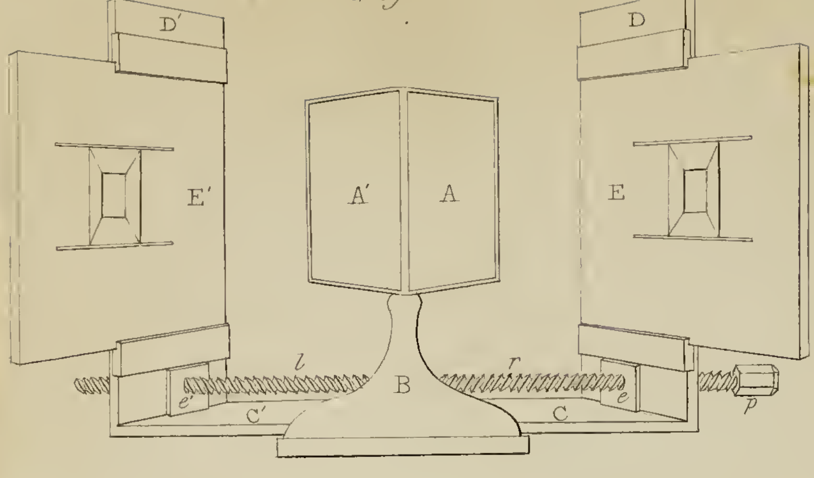
To determine the Constant Angle of the Spiral affected by any given Shell.

The common ratio of the geometrical progression according to which the widths of successive whorls increase being determined by actual admeasurement and represented by λ, we have the equation

$$\varepsilon^{2\pi\cot A}=\lambda$$

$$\therefore A=\tan^{-1}\left(\frac{2\pi}{\log_\varepsilon\lambda}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (26.)$$

Fig. 8.



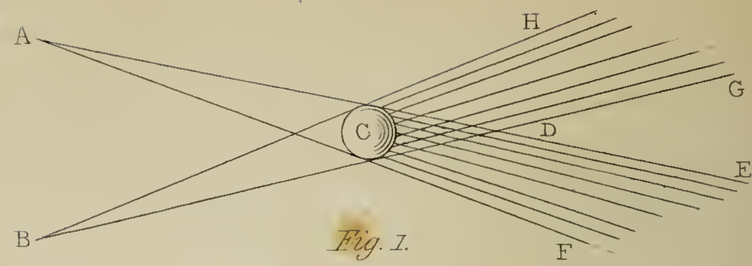


Fig. 1.

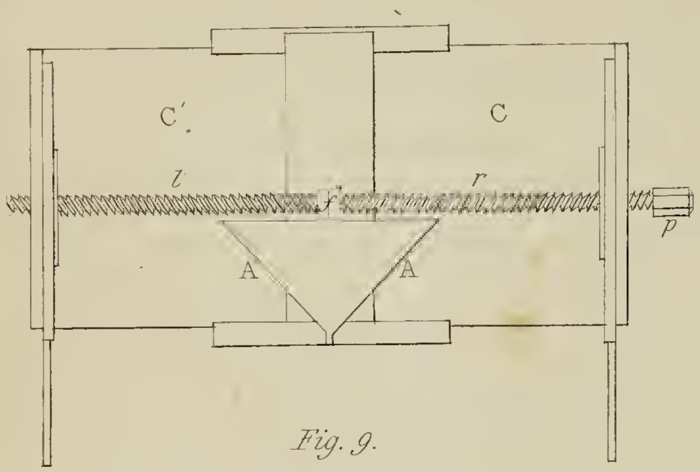


Fig. 9.

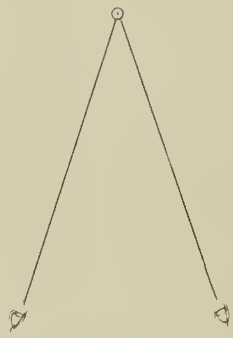


Fig. 2.

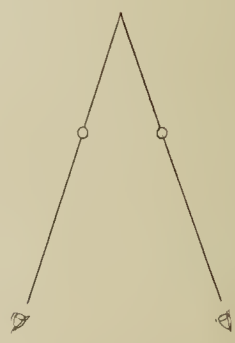


Fig. 3.

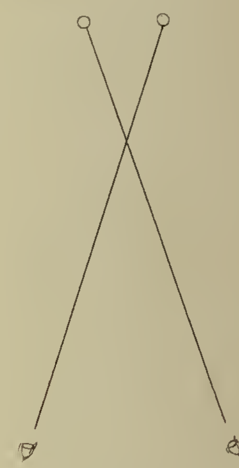


Fig. 4.

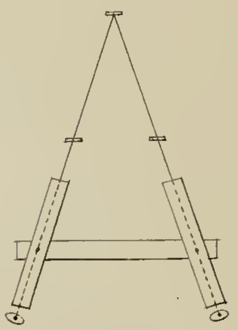


Fig. 5.

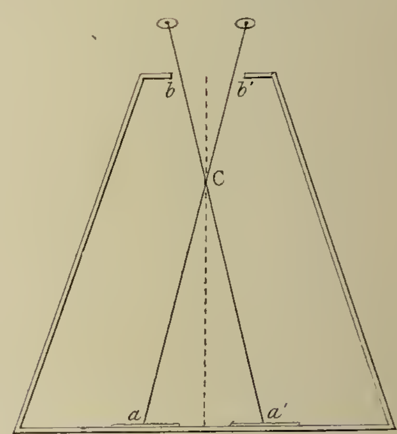


Fig. 6.

Fig. 26.

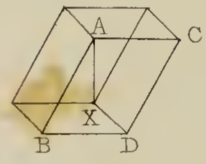


Fig. 22.

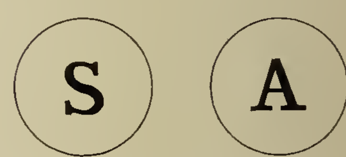


Fig. 25.

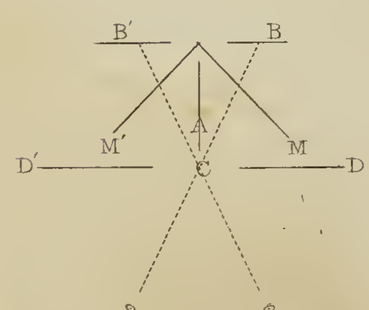


Fig. 21.

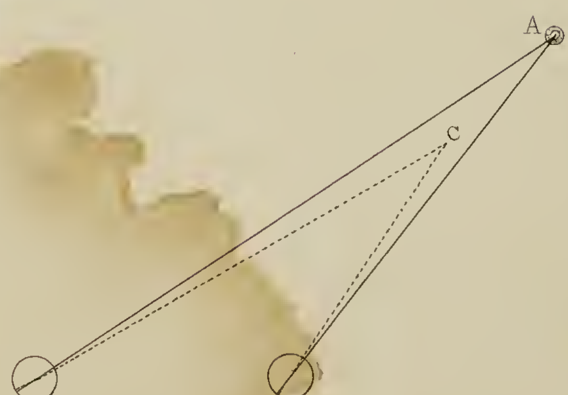


Fig. 24.

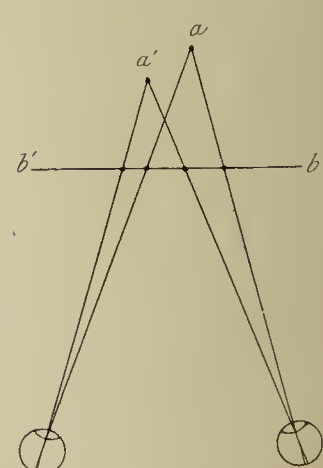


Fig. 7.

XVIII. *Contributions to the Physiology of Vision.—Part the First. On some remarkable, and hitherto unobserved, Phenomena of Binocular Vision.* *By* Charles Wheatstone, *F.R.S., Professor of Experimental Philosophy in King's College, London.*

Received and Read June 21, 1838.

§ 1.

When an object is viewed at so great a distance that the optic axes of both eyes are sensibly parallel when directed towards it, the perspective projections of it, seen by each eye separately, are similar, and the appearance to the two eyes is precisely the same as when the object is seen by one eye only. There is, in such case, no difference between the visual appearance of an object in relief and its perspective projection on a plane surface; and hence pictorial representations of distant objects, when those circumstances which would prevent or disturb the illusion are carefully excluded, may be rendered such perfect resemblances of the objects they are intended to represent as to be mistaken for them; the Diorama is an instance of this. But this similarity no longer exists when the object is placed so near the eyes that to view it the optic axes must converge; under these conditions a different perspective projection of it is seen by each eye, and these perspectives are more dissimilar as the convergence of the optic axes becomes greater. This fact may be easily verified by placing any figure of three dimensions, an outline cube for instance, at a moderate distance before the eyes, and while the head is kept perfectly steady, viewing it with each eye successively while the other is closed. Plate XI. fig. 13. represents the two perspective projections of a cube; *b* is that seen by the right eye, and *a* that presented to the left eye; the figure being supposed to be placed about seven inches immediately before the spectator.

The appearances, which are by this simple experiment rendered so obvious, may be easily inferred from the established laws of perspective; for the same object in relief is, when viewed by a different eye, seen from two points of sight at a distance from each other equal to the line joining the two eyes. Yet they seem to have escaped the attention of every philosopher and artist who has treated of the subjects of vision and perspective. I can ascribe this inattention to a phenomenon leading to the important and curious consequences, which will form the subject of the present communication, only to this circumstance; that the results being contrary to a principle which was very generally maintained by optical writers, viz. that objects can

be seen single only when their images fall on corresponding points of the two retinæ, an hypothesis which will be hereafter discussed, if the consideration ever arose in their minds, it was hastily discarded under the conviction, that if the pictures presented to the two eyes are under certain circumstances dissimilar, their differences must be so small that they need not be taken into account.

It will now be obvious why it is impossible for the artist to give a faithful representation of any near solid object, that is, to produce a painting which shall not be distinguished in the mind from the object itself. When the painting and the object are seen with both eyes, in the case of the painting two *similar* pictures are projected on the retinæ, in the case of the solid object the pictures are *dissimilar*; there is therefore an essential difference between the impressions on the organs of sensation in the two cases, and consequently between the perceptions formed in the mind; the painting therefore cannot be confounded with the solid object.

After looking over the works of many authors who might be expected to have made some remarks relating to this subject, I have been able to find but one, which is in the Trattato della Pittura of LEONARDO DA VINCI*. This great artist and ingenious philosopher observes, "that a painting, though conducted with the greatest art and finished to the last perfection, both with regard to its contours, its lights, its shadows and its colours, can never show a relievo equal to that of the natural objects, unless these be viewed at a distance and with a single eye. For," says he, "if an object C (Plate X. fig. 1.) be viewed by a single eye at A, all objects in the space behind it, included as it were in a shadow E C F cast by a candle at A, are invisible to the eye at A; but when the other eye at B is opened, part of these objects become visible to it; those only being hid from both eyes that are included, as it were, in the double shadow C D, cast by two lights at A and B, and terminated in D, the angular space E D G beyond D being always visible to both eyes. And the hidden space C D is so much the shorter, as the object C is smaller and nearer to the eyes. Thus the object C seen with both eyes becomes, as it were, transparent, according to the usual definition of a transparent thing; namely, that which hides nothing beyond it. But this cannot happen when an object, whose breadth is bigger than that of the pupil, is viewed by a single eye. The truth of this observation is therefore evident, because a painted figure intercepts all the space behind its apparent place, so as to preclude the eyes from the sight of every part of the imaginary ground behind it."

Had LEONARDO DA VINCI taken, instead of a sphere, a less simple figure for the purpose of his illustration, a cube for instance, he would not only have observed that the object obscured from each eye a different part of the more distant field of view, but the fact would also perhaps have forced itself upon his attention, that the object itself presented a different appearance to each eye. He failed to do this, and no subsequent writer within my knowledge has supplied the omission; the projection of two

* See also a Treatise of Painting, p. 178. London, 1721; and Dr. SMITH's Complete System of Optics, vol. ii. r. 244, where the passage is quoted.

Fig. 10.

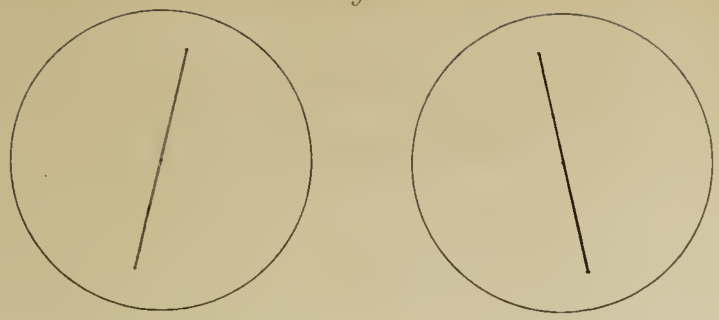


Fig. 23.

Fig. 12.

Fig. 11.

Fig. 13.

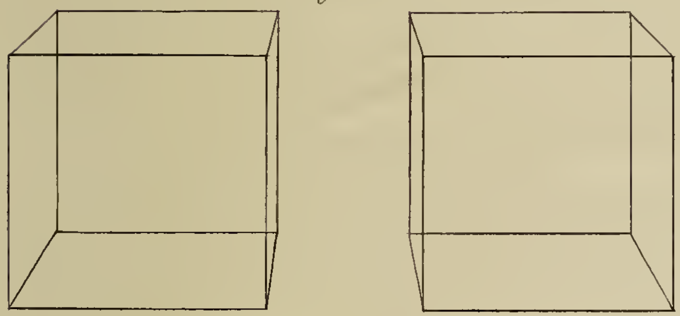


Fig. 14.

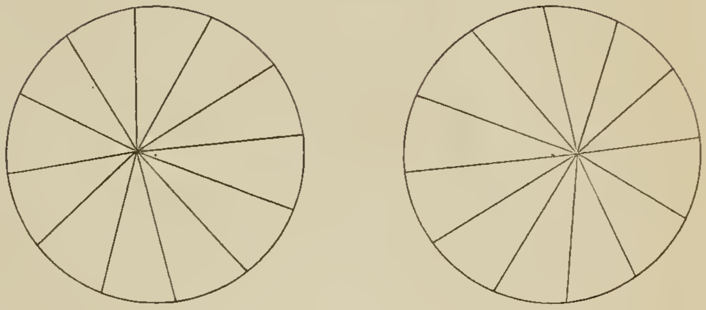


Fig. 15.

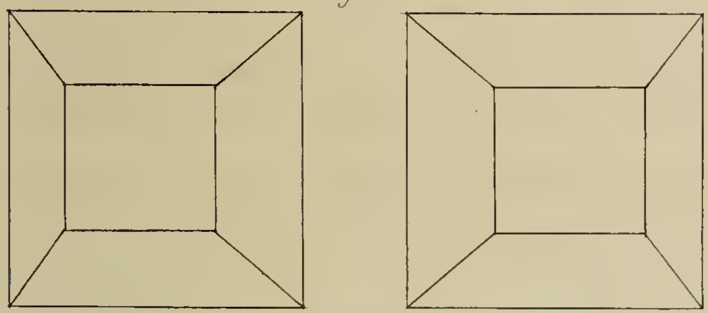


Fig. 16.

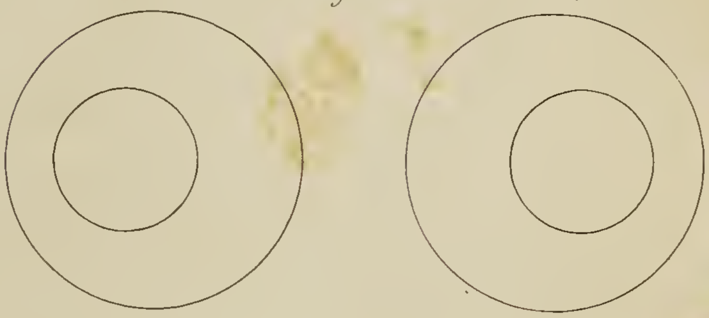


Fig. 17.

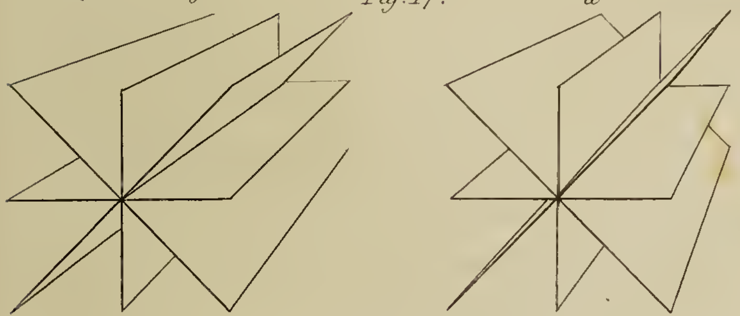


Fig. 18.

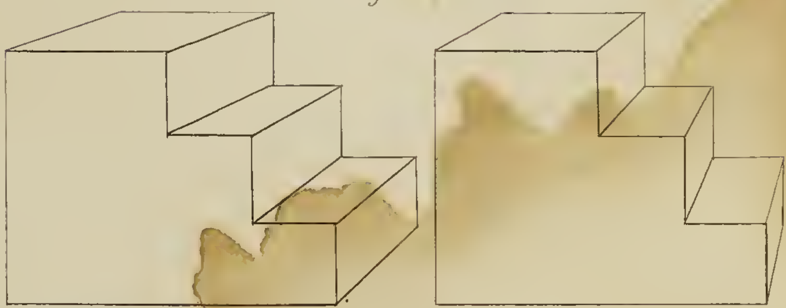


Fig. 19.

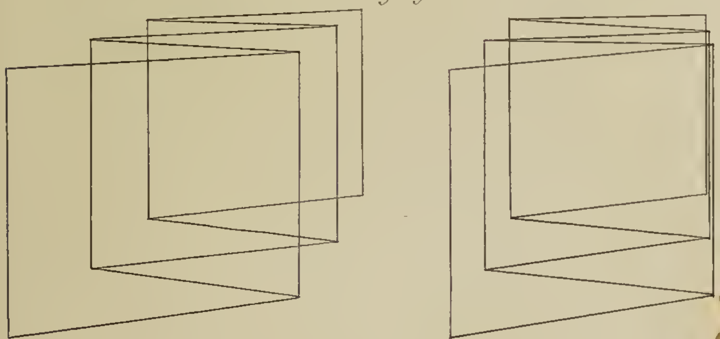
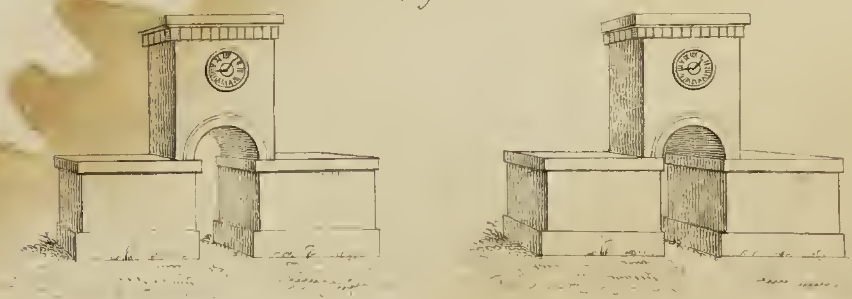


Fig. 20.



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obviously dissimilar pictures on the two retinæ when a single object is viewed, while the optic axes converge, must therefore be regarded as a new fact in the theory of vision.

§ 2.

It being thus established that the mind perceives an object of three dimensions by means of the two dissimilar pictures projected by it on the two retinæ, the following question occurs: What would be the visual effect of simultaneously presenting to each eye, instead of the object itself, its projection on a plane surface as it appears to that eye? To pursue this inquiry it is necessary that means should be contrived to make the two pictures, which must necessarily occupy different places, fall on similar parts of both retinæ. Under the ordinary circumstances of vision the object is seen at the concourse of the optic axes, and its images consequently are projected on similar parts of the two retinæ; but it is also evident that two exactly similar objects may be made to fall on similar parts of the two retinæ, if they are placed one in the direction of each optic axis, at equal distances before or beyond their intersection.

Fig. 2. represents the usual situation of an object at the intersection of the optic axes. In fig. 3. the similar objects are placed in the direction of the optic axes before their intersection, and in fig. 4. beyond it. In all these three cases the mind perceives but a single object, and refers it to the place where the optic axes meet. It will be observed, that when the eyes converge beyond the objects, as in fig. 3., the right hand object is seen by the right eye, and the left hand object by the left eye; while when the axes converge nearer than the objects, the right hand object is seen by the left eye, and conversely. As both of these modes of vision are forced and unnatural, eyes unaccustomed to such experiments require some artificial assistance. If the eyes are to converge beyond the objects, this may be afforded by a pair of tubes (fig. 5.) capable of being inclined towards each other at various angles, so as to correspond with the different convergences of the optic axes. If the eyes are to converge at a nearer distance than that at which the objects are placed, a box (fig. 6.) may be conveniently employed; the objects a a' are placed distant from each other, on a stand capable of being moved nearer the eyes if required, and the optic axes being directed towards them will cross at c, the aperture b b' allowing the visual rays from the right hand object to reach the left eye, and those from the left hand object to fall on the right eye; the coincidence of the images may be facilitated by placing the point of a needle at the point of intersection of the optic axes c, and fixing the eyes upon it. In both these instruments (figs. 5. and 6.) the lateral images are hidden from view, and much less difficulty occurs in making the images unite than when the naked eyes are employed.

Now if, instead of placing two exactly similar objects to be viewed by the eyes in either of the modes above described, the two perspective projections of the same solid object be so disposed, the mind will still perceive the object to be single, but instead of a representation on a plane surface, as each drawing appears to be when separately

viewed by that eye which is directed towards it, the observer will perceive a figure of three dimensions, the exact counterpart of the object from which the drawings were made. To make this matter clear I will mention one or two of the most simple cases.

If two vertical lines near each other, but at different distances from the spectator, be regarded first with one eye and then with the other, the distance between them when referred to the same plane will appear different; if the left hand line be nearer to the eyes, the distance as seen by the left eye will be less than the distance as seen by the right eye; fig. 7. will render this evident; *a a'* are vertical sections of the two original lines, and *b b'* the plane to which their projections are referred. Now if the two lines be drawn on two pieces of card, at the respective distances at which they appear to each eye, and these cards be afterwards viewed by either of the means above directed, the observer will no longer see two lines on a plane surface, as each card separately shows; but two lines will appear, one nearer to him than the other, precisely as the original vertical lines themselves. Again, if a straight wire be held before the eyes in such a position that one of its ends shall be nearer to the observer than the other is, each eye separately referring it to a plane perpendicular to the common axis, will see a line differently inclined; and then if lines having the same apparent inclinations be drawn on two pieces of card, and be presented to the eyes as before directed, the real position of the original line will be correctly perceived by the mind.

In the same manner the most complex figures of three dimensions may be accurately represented to the mind, by presenting their two perspective projections to the two retinæ. But I shall defer these more perfect experiments until I describe an instrument which will enable any person to observe all the phenomena in question with the greatest ease and certainty.

In the instruments above described the optic axes converge to some point in a plane before or beyond that in which the objects to be seen are situated. The adaptation of the eye, which enables us to see distinctly at different distances, and which habitually accompanies every different degree of convergence of the optic axes, does not immediately adjust itself to the new and unusual condition; and to persons not accustomed to experiments of this kind, the pictures will either not readily unite, or will appear dim and confused. Besides this, no object can be viewed according to either mode when the drawings exceed in breadth the distance of the two points of the optic axes in which their centres are placed.

These inconveniences are removed by the instrument I am about to describe; the two pictures (or rather their reflected images) are placed in it at the true concourse of the optic axes, the focal adaptation of the eye preserves its usual adjustment, the appearance of lateral images is entirely avoided, and a large field of view for each eye is obtained. The frequent reference I shall have occasion to make to this instrument, will render it convenient to give it a specific name, I therefore propose that it be called a Stereoscope, to indicate its property of representing solid figures.

§ 3.

The stereoscope is represented by figs. 8. and 9; the former being a front view, and the latter a plan of the instrument. A A′ are two plane mirrors, about four inches square, inserted in frames, and so adjusted that their backs form an angle of 90° with each other; these mirrors are fixed by their common edge against an upright B, or which was less easy to represent in the drawing, against the middle line of a vertical board, cut away in such manner as to allow the eyes to be placed before the two mirrors. C C′ are two sliding boards, to which are attached the upright boards D D′, which may thus be removed to different distances from the mirrors. In most of the experiments hereafter to be detailed, it is necessary that each upright board shall be at the same distance from the mirror which is opposite to it. To facilitate this double adjustment, I employ a right and a left-handed wooden screw, *r l*; the two ends of this compound screw pass through the nuts *e e′*, which are fixed to the lower parts of the upright boards D D′, so that by turning the screw pin *p* one way the two boards will approach, and by turning it the other they will recede from each other, one always preserving the same distance as the other from the middle line *f*. E E′ are pannels, to which the pictures are fixed in such manner that their corresponding horizontal lines shall be on the same level: these pannels are capable of sliding backwards and forwards in grooves on the upright boards D D′. The apparatus having been described, it now remains to explain the manner of using it. The observer must place his eyes as near as possible to the mirrors, the right eye before the right hand mirror, and the left eye before the left hand mirror, and he must move the sliding pannels E E′ to or from him until the two reflected images coincide at the intersection of the optic axes, and form an image of the same apparent magnitude as each of the component pictures. The pictures will indeed coincide when the sliding pannels are in a variety of different positions, and consequently when viewed under different inclinations of the optic axes; but there is only one position in which the binocular image will be immediately seen single, of its proper magnitude, and without fatigue to the eyes, because in this position only the ordinary relations between the magnitude of the pictures on the retina, the inclination of the optic axes, and the adaptation of the eye to distinct vision at different distances are preserved. The alteration in the apparent magnitude of the binocular images, when these usual relations are disturbed, will be discussed in another paper of this series, with a variety of remarkable phenomena depending thereon. In all the experiments detailed in the present memoir I shall suppose these relations to remain undisturbed, and the optic axes to converge about six or eight inches before the eyes.

If the pictures are all drawn to be seen with the same inclination of the optic axes, the apparatus may be simplified by omitting the screw *r l* and fixing the upright boards D D′ at the proper distances. The sliding pannels may also be dispensed with, and the drawings themselves be made to slide in the grooves.

§ 4.

A few pairs of outline figures, calculated to give rise to the perception of objects of three dimensions when placed in the stereoscope in the manner described, are represented from figs. 10. to 20. They are one half the linear size of the figures actually employed. As the drawings are reversed by reflection in the mirrors, I will suppose these figures to be the reflected images to which the eyes are directed in the apparatus; those marked *b* being seen by the right eye, and those marked *a* by the left eye. The drawings, it has been already explained, are two different projections of the same object seen from two points of sight, the distance between which is equal to the interval between the eyes of the observer; this interval is generally about $2\frac{1}{2}$ inches.

a and *b*, fig. 10. will, when viewed in the stereoscope, present to the mind a line in the vertical plane, with its lower end inclined towards the observer. If the two component lines be caused to turn round their centres equally in opposite directions, the resultant line will, while it appears to assume every degree of inclination to the referent plane, still seem to remain in the same vertical plane.

Fig. 11. A series of points all in the same horizontal plane, but each towards the right hand successively nearer the observer.

Fig. 12. A curved line intersecting the referent plane, and having its convexity towards the observer.

Fig. 13. A cube.

Fig. 14. A cone, having its axis perpendicular to the referent plane, and its vertex towards the observer.

Fig. 15. The frustum of a square pyramid; its axis perpendicular to the referent plane, and its base furthest from the eye.

Fig. 16. Two circles at different distances from the eyes, their centres in the same perpendicular, forming the outline of the frustum of a cone.

The other figures require no observation.

For the purposes of illustration I have employed only outline figures, for had either shading or colouring been introduced it might be supposed that the effect was wholly or in part due to these circumstances, whereas by leaving them out of consideration no room is left to doubt that the entire effect of relief is owing to the simultaneous perception of the two monocular projections, one on each retina. But if it be required to obtain the most faithful resemblances of real objects, shadowing and colouring may properly be employed to heighten the effects. Careful attention would enable an artist to draw and paint the two component pictures, so as to present to the mind of the observer, in the resultant perception, perfect identity with the object represented. Flowers, crystals, busts, vases, instruments of various kinds, &c., might thus be represented so as not to be distinguished by sight from the real objects themselves.

It is worthy of remark, that the process by which we thus become acquainted with the real forms of solid objects, is precisely that which is employed in descriptive geometry, an important science we owe to the genius of MONGE, but which is little studied or known in this country. In this science, the position of a point, a right line or a curve, and consequently of any figure whatever, is completely determined by assigning its projections on two fixed planes, the situations of which are known, and which are not parallel to each other. In the problems of descriptive geometry the two referent planes are generally assumed to be at right angles to each other, but in binocular vision the inclination of these planes is less according as the angle made at the concourse of the optic axes is less; thus the same solid object is represented to the mind by different pairs of monocular pictures, according as they are placed at a different distance before the eyes, and the perception of these differences (though we seem to be unconscious of them) may assist in suggesting to the mind the distance of the object. The more inclined to each other the referent planes are, with the greater accuracy are the various points of the projections referred to their proper places; and it appears to be a useful provision that the real forms of those objects which are nearest to us are thus more determinately apprehended than those which are more distant.

§ 5.

A very singular effect is produced when the drawing originally intended to be seen by the right eye is placed at the left hand side of the stereoscope, and that designed to be seen by the left eye is placed on its right hand side. A figure of three dimensions, as bold in relief as before, is perceived, but it has a different form from that which is seen when the drawings are in their proper places. There is a certain relation between the proper figure and this, which I shall call its *converse* figure. Those points which are nearest the observer in the proper figure are the most remote from him in the converse figure, and *vice versâ,* so that the figure is, as it were, inverted; but it is not an exact inversion, for the near parts of the converse figure appear smaller, and the remote parts larger than the same parts before the inversion. Hence the drawings which, properly placed, occasion a cube to be perceived, when changed in the manner described, represent the frustum of a square pyramid with its base remote from the eye: the cause of this is easy to understand.

This conversion of relief may be shown by all the pairs of drawings from fig. 10. to 19. In the case of simple figures like these the converse figure is as readily apprehended as the original one, because it is generally a figure of as frequent occurrence; but in the case of a more complicated figure, an architectural design, for instance, the mind, unaccustomed to perceive its converse, because it never occurs in nature, can find no meaning in it.

§ 6.

The same image is depicted on the retina by an object of three dimensions as by its projection on a plane surface, provided the point of sight remain in both cases the same. There should be, therefore, no difference in the binocular appearance of two drawings, one presented to each eye, and of two real objects so presented to the two eyes that their projections on the retina shall be the same as those arising from the drawings. The following experiments will prove the justness of this inference.

I procured several pairs of skeleton figures, i. e. outline figures of three dimensions, formed either of iron wire or of ebony beading about one tenth of an inch in thickness. The pair I most frequently employed consisted of two cubes, whose sides were three inches in length. When I placed these skeleton figures on stands before the two mirrors of the stereoscope, the following effects were produced, according as their relative positions were changed. 1st. When they were so placed that the pictures which their reflected images projected on the two retinæ were precisely the same as those which would have been projected by a cube placed at the concourse of the optic axes, a cube in relief appeared before the eyes. 2ndly. When they were so placed that their reflected images projected exactly similar pictures on the two retinæ, all effect of relief was destroyed, and the compound appearance was that of an outline representation on a plane surface. 3rdly. When the cubes were so placed that the reflected image of one projected on the left retina the same picture as in the first case was projected on the right retina, and conversely, the converse figure in relief appeared.

§ 7.

If a symmetrical object, that is one whose right and left sides are exactly similar to each other but inverted, be placed so that any point in the plane which divides it into these two halves is equally distant from the two eyes, its two monocular projections are, it is easy to see, inverted fac-similes of each other. Thus fig. 15, *a* and *b* are symmetrical monocular projections of the frustum of a four-sided pyramid, and figs. 13. 14. 16. are corresponding projections of other symmetrical objects. This being kept in view, I will describe an experiment which, had it been casually observed previous to the knowledge of the principles developed in this paper, would have appeared an inexplicable optical illusion.

M and M′ (fig. 21.) are two mirrors, inclined so that their *faces* form an angle of 90° with each other. Between them in the bisecting plane is placed a plane outline figure, such as fig. 15 *a*, made of card all parts but the lines being cut away, or of wire. A reflected image of this outline, placed at A, will appear behind each mirror at B and B′, and one of these images will be the inversion of the other. If the eyes be made to converge at C, it is obvious that these two reflected images will fall on corresponding parts of the two retinæ, and a figure of three dimensions will be perceived; if the outline placed in the bisecting plane be reversed, the converse skeleton

form will appear; in both these experiments we have the singular phenomenon of the conversion of a single plane outline into a figure of three dimensions. To render the binocular object more distinct, concave lenses may be applied to the eyes; and to prevent the two lateral images from being seen, screens may be placed at D and D′.

§ 8.

An effect of binocular perspective may be remarked in a plate of metal, the surface of which has been made smooth by turning it in a lathe. When a single candle is brought near such a plate, a line of light appears standing out from it, one half being above, and the other half below the surface; the position and inclination of this line changes with the situation of the light and of the observer, but it always passes through the centre of the plate. On closing the left eye the relief disappears, and the luminous line coincides with one of the diameters of the plate; on closing the right eye the line appears equally in the plane of the surface, but coincides with another diameter; on opening both eyes it instantly starts into relief*. The case here is exactly analogous to the vision of two inclined lines (fig. 10.) when each is presented to a different eye in the stereoscope. It is curious, that an effect like this, which must have been seen thousands of times, should never have attracted sufficient attention to have been made the subject of philosophic observation. It was one of the earliest facts which drew my attention to the subject I am now treating.

Dr. Smith† was very much puzzled by an effect of binocular perspective which he observed, but was unable to explain. He opened a pair of compasses, and while he held the joint in his hand, and the points outwards and equidistant from his eyes, and somewhat higher than the joint, he looked at a more distant point; the compasses appeared double. He then compressed the legs until the two inner points coincided; having done this the two inner legs also entirely coincided, and bisected the angle formed by the outward ones, appearing longer and thicker than they did, and reaching from the hand to the remotest object in view. The explanation offered by Dr. Smith accounts only for the coincidence of the points of the compasses, not for that of the entire leg. The effect in question is best seen by employing a pair of straight wires, about a foot in length. A similar observation, made with two flat rulers, and afterwards with silk threads, induced Dr. Wells to propose a new theory of visible direction in order to explain it, so inexplicable did it seem to him by any of the received theories.

* The luminous line seen by a single eye arises from the reflection of the light from each of the concentric circles produced in the operation of turning; when the plate is not large the arrangement of these successive reflections does not differ from a straight line.

† System of Optics, vol. ii. p. 388. and r. 526.

§ 9.

The preceding experiments render it evident that there is an essential difference in the appearance of objects when seen with two eyes, and when only one eye is employed, and that the most vivid belief of the solidity of an object of three dimensions arises from two different perspective projections of it being simultaneously presented to the mind. How happens it then, it may be asked, that persons who see with only one eye form correct notions of solid objects, and never mistake them for pictures? and how happens it also, that a person having the perfect use of both eyes, perceives no difference in objects around him when he shuts one of them? To explain these apparent difficulties, it must be kept in mind, that although the simultaneous vision of two dissimilar pictures suggests the relief of objects in the most vivid manner, yet there are other signs which suggest the same ideas to the mind, which, though more ambiguous than the former, become less liable to lead the judgment astray in proportion to the extent of our previous experience. The vividness of relief arising from the projection of two dissimilar pictures, one on each retina, becomes less and less as the object is seen at a greater distance before the eyes, and entirely ceases when it is so distant that the optic axes are parallel while regarding it. We see with both eyes all objects beyond this distance precisely as we see near objects with a single eye; for the pictures on the two retinæ are then exactly similar, and the mind appreciates no difference whether two identical pictures fall on corresponding parts of the two retinæ, or whether one eye is impressed with only one of these pictures. A person deprived of the sight of one eye sees therefore all external objects, near and remote, as a person with both eyes sees remote objects only, but that vivid effect arising from the binocular vision of near objects is not perceived by the former; to supply this deficiency he has recourse unconsciously to other means of acquiring more accurate information. The motion of the head is the principal means he employs. That the required knowledge may be thus obtained will be evident from the following considerations. The mind associates with the idea of a solid object every different projection of it which experience has hitherto afforded; a single projection may be ambiguous, from its being also one of the projections of a picture, or of a different solid object; but when different projections of the same object are successively presented, they cannot all belong to another object, and the form to which they belong is completely characterized. While the object remains fixed, at every movement of the head it is viewed from a different point of sight, and the picture on the retina consequently continually changes.

Every one must be aware how greatly the perspective effect of a picture is enhanced by looking at it with only one eye, especially when a tube is employed to exclude the vision of adjacent objects, whose presence might disturb the illusion. Seen under such circumstances from the proper point of sight, the picture projects the same lines, shades and colours on the retina, as the more distant scene which it represents

would do were it substituted for it. The appearance which would make us certain that it is a picture is excluded from the sight, and the imagination has room to be active. Several of the older writers erroneously attributed this apparent superiority of monocular vision to the concentration of the visual power in a single eye *.

There is a well-known and very striking illusion of perspective which deserves a passing remark, because the reason of the effect does not appear to be generally understood. When a perspective of a building is projected on a horizontal plane, so that the point of sight is in a line greatly inclined towards the plane, the building appears to a single eye placed at the point of sight to be in bold relief, and the illusion is almost as perfect as in the binocular experiments described in §§ 2, 3, 4. This effect wholly arises from the unusual projection, which suggests to the mind more readily the object itself than the drawing of it; for we are accustomed to see real objects in almost every point of view, but perspective representations being generally made in a vertical plane with the point of sight in a line perpendicular to the plane of projection, we are less familiar with the appearance of other projections. Any other unusual projection will produce the same effect.

§ 10.

If we look with a single eye at the drawing of a solid geometrical figure, it may be imagined to be the representation of either of two dissimilar solid figures, the figure intended to be represented, or its converse figure (§ 5.). If the former is a very usual, and the latter a very unusual figure, the imagination will fix itself on the original without wandering to the converse figure; but if both are of ordinary occurrence, which is generally the case with regard to simple forms, a singular phenomenon takes place; it is perceived at one time distinctly as one of these figures, at another time as the other, and while one figure continues it is not in the power of the will to change it immediately.

The same phenomenon takes place, though less decidedly, when the drawing is seen with both eyes. Many of my readers will call to mind the puzzling effect of some of the diagrams annexed to the problems of the eleventh book of Euclid; which, when they were attentively looked at, changed in an arbitrary manner from one solid figure to another, and would obstinately continue to present the converse figures when the real figures alone were wanted. This perplexing illusion must be of common occurrence, but I have only found one recorded observation relating to the subject. It is by Professor Necker of Geneva, and I shall quote it in his own words from the Philosophical Magazine, Third Series, vol. i. p. 337.

"The object I have now to call your attention to is an observation which has often

* "We see more exquisitely with one eye shut than with both, because the vital spirits thus unite themselves the more, and become the stronger: for we may find by looking in a glass whilst we shut one eye, that the pupil of the other dilates."—Lord Bacon's Works, Sylva Sylvarum, art. Vision.

occurred to me while examining figures and engraved plates of crystalline forms; I mean a sudden and involuntary change in the apparent position of a crystal or solid represented in an engraved figure. What I mean will be more easily understood from the figure annexed (fig. 22.). The rhomboid A X is drawn so that the solid angle A should be seen the nearest to the spectator, and the solid angle X the farthest from him, and that the face A C D B should be the foremost, while the face X D C is behind. But in looking repeatedly at the same figure, you will perceive that at times the apparent position of the rhomboid is so changed that the solid angle X will appear the nearest, and the solid angle A the farthest; and that the face A C D B will recede behind the face X D C, which will come forward, which effect gives to the whole solid a quite contrary apparent inclination."

Professor Necker attributes this alteration of appearance, not to a mental operation, but to an involuntary change in the adjustment of the eye for obtaining distinct vision. He supposed that whenever the point of distinct vision on the retina is directed on the angle A, for instance, this angle seen more distinctly than the others is naturally supposed to be nearer and foremost, while the other angles seen indistinctly are supposed to be farther and behind, and that the reverse takes place when the point of distinct vision is brought to bear on the angle X.

That this is not the true explanation, is evident from three circumstances: in the first place, the two points A and X being both at the same distance from the eyes, the same alteration of adjustment which would make one of them indistinct would make the other so; secondly, the figure will undergo the same changes whether the focal distance of the eye be adjusted to a point before or beyond the plane in which the figure is drawn; and thirdly, the change of figure frequently occurs while the eye continues to look at the same angle. The effect seems entirely to depend on our mental contemplation of the figure intended to be represented, or of its converse. By following the lines with the eye with a clear idea of the solid figure we are describing, it may be fixed for any length of time; but it requires practice to do this or to change the figure at will. As I have before observed, these effects are far more obvious when the figures are regarded with one eye only.

No illusion of this kind can take place when an object of three dimensions is seen with both eyes while the optic axes make a sensible angle with each other, because the appearance of the two dissimilar images, one to each eye, prevents the possibility of mistake. But if we regard an object at such a distance that its two projections are sensibly identical, and if this projection be capable of a double interpretation, the illusion may occur. Thus a placard on a pole carried in the streets, with one of its sides inclined towards the observer, will, when he is distant from it, frequently appear inclined in a contrary direction. Many analogous instances might be adduced, but this will suffice to call others to mind; it must however be observed, that when shadows, or other means capable of determining the judgement are present, these fallacies do not arise.

§ 11.

The same indetermination of judgement which causes a drawing to be perceived by the mind at different times as two different figures, frequently gives rise to a false perception when objects in relief are regarded with a single eye. The apparent conversion of a cameo into an intaglio, and of an intaglio into a cameo, is a well-known instance of this fallacy in vision; but the fact does not appear to me to have been correctly explained, nor the conditions under which it occurs to have been properly stated.

This curious illusion, which has been the subject of much attention, was first observed at one of the early meetings of the Royal Society*. Several of the members looking through a compound microscope of a new construction at a guinea, some of them imagined the image to be depressed, while others thought it to be embossed, as it really was. Professor Gmelin, of Wurtemburg, published a paper on the same subject in the Philosophical Transactions for 1745; his experiments were made with telescopes and compound microscopes which inverted the images; and he observed that the conversion of relief appeared in some cases and not in others, at some times and not at others, and to some eyes also and not to others. He endeavoured to ascertain some of the conditions of the two appearances; "but why these things should so happen," says he, "I do not pretend to determine."

Sir David Brewster accounts for the fallacy in the following manner†:—"A hollow seal being illuminated by a window or a candle, its shaded side is of course on the same side with the light. If we now invert the seal with one or more lenses, so that it may look in the opposite direction, it will appear to the eye with the shaded side furthest from the window. But as we know that the window is still on our left hand, and as every body with its shaded side furthest from the light must necessarily be convex or protuberant, we immediately believe that the hollow seal is now a cameo or bas-relief. The proof which the eye thus receives of the seal being raised, overcomes the evidence of its being hollow, derived from our actual knowledge and from the sense of touch. In this experiment the deception takes place from our knowing the real direction of the light which falls on the seal; for if the place of the window, with respect to the seal, had been inverted as well as the seal itself, the illusion could not have taken place. The illusion, therefore, under our consideration is the result of an operation of our own minds, whereby we judge of the forms of bodies by the knowledge we have acquired of light and shadow. Hence the illusion depends on the accuracy and extent of our knowledge on this subject; and while some persons are under its influence, others are entirely insensible to it."

These considerations do not fully explain the phenomenon, for they suppose that the image must be inverted, and that the light must fall in a particular direction; but the conversion of relief will still take place when the object is viewed through an

* Birch's History, vol. ii. p. 348.

† Natural Magic, p. 100.

open tube without any lenses to invert it, and also when it is equally illuminated in all parts. The true explanation I believe to be the following. If we suppose a cameo and an intaglio of the same object, the elevations of the one corresponding exactly to the depressions of the other, it is easy to show that the projection of either on the retina is sensibly the same. When the cameo or the intaglio is seen with both eyes, it is impossible to mistake an elevation for a depression, for reasons which have been already amply explained; but when either is seen with one eye only, the most certain guide of our judgement, viz. the presentation of a different picture to each eye, is wanting; the imagination therefore supplies the deficiency, and we conceive the object to be raised or depressed according to the dictates of this faculty. No doubt in such cases our judgement is in a great degree influenced by accessory circumstances, and the intaglio or the relief may sometimes present itself according to our previous knowledge of the direction in which the shadows ought to appear; but the real cause of the phenomenon is to be found in the indetermination of the judgement arising from our more perfect means of judging being absent.

Observers with the microscope must be particularly on their guard against illusions of this kind. Raspail observes* that the hollow pyramidal arrangement of the crystals of muriate of soda appears, when seen through a microscope, like a striated pyramid in relief. He recommends two modes of correcting the illusion. The first is to bring successively to the focus of the instrument the different parts of the crystal; if the pyramid be in relief, the point will arrive at the focus sooner than the base will; if the pyramid be hollow, the contrary will take place. The second mode is to project a strong light on the pyramid in the field of view of the microscope, and to observe which sides of the crystal are illuminated, taking however the inversion of the image into consideration if a compound microscope be employed.

The inversion of relief is very striking when a skeleton cube is looked at with one eye, and the following singular results may in this case be observed. So long as the mind perceives the cube, however the figure be turned about, its various appearances will be but different representations of the same object, and the same primitive form will be suggested to the mind by all of them: but it is not so if the converse figure fixes the attention; the series of successive projections cannot then be referred to any figure to which they are all common, and the skeleton figure will appear to be continually undergoing a change of shape.

§ 12.

I have given ample proof that objects whose pictures do not fall on corresponding points of the two retinæ may still appear single. I will now adduce an experiment which proves that similar pictures falling on corresponding points of the two retinæ may appear double and in different places.

Present, in the stereoscope, to the right eye a vertical line, and to the left eye a

* Nouveau Système de Chimie Organique, 2me edit. t. 1. p. 333.

line inclined some degrees from the perpendicular (fig. 23.); the observer will then perceive, as formerly explained, a line, the extremities of which appear at different distances before the eyes. Draw on the left hand figure a faint vertical line exactly corresponding in position and length to that presented to the right eye, and let the two lines of this left hand figure intersect each other at their centres. Looking now at these two drawings in the stereoscope, the two strong lines, each seen by a different eye, will coincide, and the resultant perspective line will appear to occupy the same place as before; but the faint line which now falls on a line of the left retina, which corresponds with the line of the right retina on which one of the coinciding strong lines, viz. the vertical one, falls, appears in a different place. The place this faint line apparently occupies is the intersection of that plane of visual direction of the left eye in which it is situated, with the plane of visual direction of the right eye, which contains the strong vertical line.

This experiment affords another proof that there is no necessary physiological connection between the corresponding points of the two retinæ,—a doctrine which has been maintained by so many authors.

§ 13. *Binocular Vision of Images of different Magnitudes.*

We will now inquire what effect results from presenting similar images, differing only in magnitude, to analogous parts of the two retinæ. For this purpose two squares or circles, differing obviously but not extravagantly in size, may be drawn on two separate pieces of paper, and placed in the stereoscope so that the reflected image of each shall be equally distant from the eye by which it is regarded. It will then be seen that, notwithstanding this difference, they coalesce and occasion a single resultant perception. The limit of the difference of size within which the single appearance subsists may be ascertained by employing two images of equal magnitude, and causing one of them to recede from the eye while the other remains at a constant distance; this is effected merely by pulling out the sliding board C (fig. 8.) while the other C′ remains fixed, the screw having previously been removed.

Though the single appearance of two images of different size is by this experiment demonstrated, the observer is unable to perceive what difference exists between the apparent magnitude of the binocular image and that of the two monocular images; to determine this point the stereoscope must be dispensed with, and the experiment so arranged that all three shall be simultaneously seen; which may be done in the following manner:—The two drawings being placed side by side on a plane before the eyes, the optic axes must be made to converge to a nearer point as at fig. 4., or to a more distant one as at fig. 3., until the three images are seen at the same time, the binocular image in the middle, and the monocular images at each side. It will thus be seen that the binocular image is apparently intermediate in size between the two monocular ones.

If the pictures be too unequal in magnitude, the binocular coincidence does not

take place. It appears that if the inequality of the pictures be greater than the difference which exists between the two projections of the same object when seen in the most oblique position of the eyes (*i. e.* both turned to the extreme right or to the extreme left), ordinarily employed, they do not coalesce. Were it not for the binocular coincidence of two images of different magnitude, objects would appear single only when the optic axes converge immediately forwards; for it is only when the converging visual lines form equal angles with the visual base (the line joining the centres of the two eyes) as at fig. 2., that the two pictures can be of equal magnitude; but when they form different angles with it, as at fig. 24., the distance from the object to each eye is different, and consequently the picture projected on each retina has a different magnitude. If a piece of money be held in the position *a*, (fig. 24.) while the optic axes converge to a nearer point *c*, it will appear double, and that seen by the left eye will be evidently smaller than the other.

§ 14. *Phenomena which are observed when objects of different forms are simultaneously presented to corresponding parts of the two retinæ.*

If we regard a picture with the right eye alone for a considerable length of time it will be constantly perceived; if we look at another and dissimilar picture with the left eye alone its effect will be equally permanent; it might therefore be expected, that if each of these pictures were presented to its corresponding eye at the same time the two would appear permanently superposed on each other. This, however, contrary to expectation, is not the case.

If *a* and *b* (fig. 25.) are each presented at the same time to a different eye, the common border will remain constant, while the letter within it will change alternately from that which would be perceived by the right eye alone to that which would be perceived by the left eye alone. At the moment of change the letter which has just been seen breaks into fragments, while fragments of the letter which is about to appear mingle with them, and are immediately after replaced by the entire letter. It does not appear to be in the power of the will to determine the appearance of either of the letters, but the duration of the appearance seems to depend on causes which are under our control: thus if the two pictures be equally illuminated, the alternations appear in general of equal duration; but if one picture be more illuminated than the other, that which is less so will be perceived during a shorter time. I have generally made this experiment with the apparatus, fig. 6. When complex pictures are employed in the stereoscope, various parts of them alternate differently.

There are some facts intimately connected with the subject of the present article which have already been frequently observed. I allude to the experiments, first made by Du Tour, in which two different colours are presented to corresponding parts of the two retinæ. If a blue disc be presented to the right eye and a yellow disc to the corresponding part of the left eye, instead of a green disc which would appear if these

two colours had mingled before their arrival at a single eye, the mind will perceive the two colours distinctly one or the other alternately predominating either partially or wholly over the disc. In the same manner the mind perceives no trace of violet when red is presented to one eye and blue to the other, nor any vestige of orange when red and yellow are separately presented in a similar manner. These experiments may be conveniently repeated by placing the coloured discs in the stereoscope, but they have been most usually made by looking at a white object through differently coloured glasses, one applied to each eye.

In some authors we find it stated, contrary to fact, that if similar objects of different colour be presented one to each eye, the appearance will be that compounded of the two colours. Dr. Reid* and Janin are among the writers who have fallen into this inconsiderate error, which arose no doubt from their deciding according to previous notions, instead of ascertaining by experiment what actually does happen.

§ 15.

No question relating to vision has been so much debated as the cause of the single appearance of objects seen by both eyes. I shall in the present section give a slight review of the various theories which have been advanced by philosophers to account for this phenomenon, in order that the remarks I have to make in the succeeding section may be properly understood.

The law of visible direction for monocular vision has been variously stated by different optical writers. Some have maintained with Drs. Reid and Porterfield, that every external point is seen in the direction of a line passing from its picture on the retina through the centre of the eye; while others have supposed with Dr. Smith that the visible direction of an object coincides with the visual ray, or the principal ray of the pencil which flows from it to the eye. D'Alembert, furnished with imperfect data respecting the refractive densities of the humours of the eye, calculated that the apparent magnitudes of objects would differ widely on the two suppositions, and concluded that the visible point of an object was not seen in either of these directions, but sensibly in the direction of a line joining the point itself and its image on the retina; but he acknowledged that he could assign no reason for this law. Sir David Brewster, provided with more accurate data, has shown that these three lines so nearly coincide with each other, that "at an inclination of 30°, a line perpendicular to the point of impression on the retina passes through the common centre, and does not deviate from the real line of visible direction more than half a degree, a quantity too small to interfere with the purposes of vision." We may, therefore, assume in all our future reasonings the truth of the following definition given by this eminent philosopher:—"As the interior eye-ball is as nearly as possible a perfect sphere, lines perpendicular to the surface of the retina must all pass through one single point,

* Enquiry, Sect. xiii.

namely the centre of its spherical surface. This one point may be called the centre of visible direction, because every point of a visible object will be seen in the direction of a line drawn from this centre to the visible point."

It is obvious, that the result of any attempt to explain the single appearance of objects to both eyes, or, in other words, the law of visible direction for binocular vision, ought to contain nothing inconsistent with the law of visible direction for monocular vision.

It was the opinion of AGUILONIUS, that all objects seen at the same glance with both eyes appear to be in the plane of the horopter. The horopter he defines to be a line drawn through the point of intersection of the optic axes, and parallel to the line joining the centres of the two eyes; the plane of the horopter to be a plane passing through this line at right angles to that of the optic axes. All objects which are in this plane, must, according to him, appear single because the lines of direction in which any point of an object is seen coincide only in this plane and nowhere else; and as these lines can meet each other only in one point, it follows from the hypothesis, that all objects not in the plane of the horopter must appear double, because their lines of direction intersect each other, either before or after they pass through it. This opinion was also maintained by DECHALES and PORTERFIELD. That it is erroneous, I have given, I think, sufficient proof, in showing that, when the optic axes converge to any point, objects before or beyond the plane of the horopter are under certain circumstances equally seen single as those in that plane.

Dr. WELLS's "new theory of visible direction" was a modification of the preceding hypothesis. This acute writer held with AGUILONIUS, that objects are seen single only when they are in the plane of the horopter, and consequently that they appear double when they are either before or beyond it; but he attempted to make this single appearance of objects only in the plane of the horopter to depend on other principles, from which he deduced, contrary to AGUILONIUS, that the objects which are doubled do not appear in the plane of the horopter, but in other places which are determined by these principles. Dr. WELLS was led to his new theory by a fact which he accidentally observed, and which he could not reconcile with any existing theory of visible direction; this fact had, though he was unaware of it, been previously noticed by Dr. SMITH; it is already mentioned in § 8., and is the only instance of binocular vision of relief which I have found recorded previous to my own investigations. So little does Dr. WELLS's theory appear to have been understood, that no subsequent writer has attempted either to confirm or disprove his opinions. It would be useless here to discuss the principles of this theory, which was framed to account for an anomalous individual fact, since it is inconsistent with the general rules on which that fact has been now shown to depend. Notwithstanding these erroneous views, the "essay upon single vision with two eyes" contains many valuable experiments and remarks, the truth of which are independent of the theory they were intended to illustrate.

The theory which has obtained greatest currency is that which assumes that an object is seen single because its pictures fall on corresponding points of the two retinæ, that is on points which are similarly situated with respect to the two centres both in distance and position. This theory supposes that the pictures projected on the retinæ are exactly similar to each other, corresponding points of the two pictures falling on corresponding points of the two retinæ. Authors who agree with regard to this property, differ widely in explaining why objects are seen in the same place, or single, according to this law. Dr. SMITH makes it to depend entirely on custom, and explains why the eyes are habitually directed towards an object so that its pictures fall on corresponding parts in the following manner:—"When we view an object steadily, we have acquired a habit of directing the optic axes to the point in view; because its pictures falling upon the middle points of the retinas, are then distincter than if they fell upon any other places; and since the pictures of the whole object are equal to one another, and are both inverted with respect to the optic axes, it follows that the pictures of any collateral point are painted upon corresponding points of the retinas."

Dr. REID, after a long dissertation on the subject, concludes, "that by an original property of human eyes, objects painted upon the centres of the two retinæ, or upon points similarly situated with regard to the centres, appear in the same visible place; that the most plausible attempts to account for this property of the eyes have been unsuccessful; and therefore, that it must be either a primary law of our constitution, or the consequence of some more general law which is not yet discovered."

Other writers who have admitted this principle have regarded it as arising from anatomical structure and dependent on connexion of nervous fibres; among these stand the names of GALEN, Dr. BRIGGS, Sir ISAAC NEWTON, ROHAULT, Dr. HARTLEY, Dr. WOLLASTON and Professor MÜLLER.

Many of the supporters of the theory of corresponding points have thought, or rather have admitted, *without thinking*, that it was not inconsistent with the law of AGUILONIUS; but very little reflection will show that both cannot be maintained together; for corresponding lines of visible direction, that is, lines terminating in corresponding points of the two retinæ, cannot meet in the plane of the horopter unless the optic axes be parallel, and the plane be at an infinite distance before the eyes. Some of the modern German writers* have inquired what is the curve in which objects appear single while the optic axes are directed to a given point, on the hypothesis that objects are seen single only when they fall on corresponding points of the two retinæ. An elegant proposition has resulted from their investigations, which I shall need no apology for introducing in this place, since it has not yet been mentioned in any English work.

R and L (fig. 26.) are the two eyes; C A, C′ A the optic axes converging to the

* *Tortual,* die Sinne des Menschen. Münster, 1827. *Bartels,* Beitrage zur Physiologie der Gesichtssinnes. Berlin, 1834.

point A; and C A B C′ is a circle drawn through the point of convergence A and the centres of visible direction C C′. If any point be taken in the circumference of this circle, and lines be drawn from it through the centres of the two eyes C C′, these lines will fall on corresponding points of the two retinæ D D′; for the angles A C B, A C′ B being equal, the angles D C E, D C′ E are also equal; therefore any point placed in the circumference of the circle C A B C′ will, according to the hypothesis, appear single while the optic axes are directed to A, or any other part in it.

I will mention two other properties of this binocular circle: 1st. The arc subtended by two points on its circumference contains double the number of degrees of the arc subtended by the pictures of these points on either retina, so that objects which occupy 180° of the supposed circle of single vision are painted on a portion of the retina extended over 90° only; for the angle D C E or D C′ E being at the centre, and the angle B C A or B C′ A at the circumference of a circle, this consequence follows. 2ndly. To whatever point of the circumference of the circle the optic axes be made to converge, they will form the same angle with each other; for the angles C A C′, C B C are equal.

In the eye itself, the centre of visible direction, or the point at which the principal rays cross each other, is, according to Dr. Young and other eminent optical writers, at the same time the centre of the spherical surface of the retina, and that of the lesser spherical surface of the cornea; in the diagram (fig. 26.), to simplify the consideration of the problem, R and L represent only the circle of curvature of the bottom of the retina, but the reasoning is equally true in both cases.

The same reasons, founded on the experiments in this memoir, which disprove the theory of Aguilonius, induce me to reject the law of corresponding points as an accurate expression of the phenomena of single vision. According to the former, objects can appear single only in the plane of the horopter; according to the latter, only when they are in the circle of single vision; both positions are inconsistent with the binocular vision of objects in relief, the points of which they consist appearing single though they are at different distances before the eyes. I have already proved that the assumption made by all the maintainers of the theory of corresponding points, namely that the two pictures projected by any object in the retinæ are exactly similar, is quite contrary to fact in every case except that in which the optic axes are parallel.

Gassendus, Porta, Tacquet and Gall maintained, that we see with only one eye at a time though both remain open, one according to them being relaxed and inattentive to objects while the other is upon the stretch. It is a sufficient refutation of this hypothesis, that we see an object double when one of the optic axes is displaced either by squinting or by pressure on the eye-ball with the finger; if we saw with only one eye, one object only should under such circumstances be seen. Again, in many cases which I have already explained, the simultaneous affection of the two retinæ excites a different idea in the mind to that consequent on either of the single impressions, the latter giving rise to the idea of a representation on a plane surface,

the former to that of an object in relief; these things could not occur did we see with only one eye at a time.

Du Tour* held that though we might occasionally see at the same time with both eyes, yet the mind cannot be affected simultaneously by two corresponding points of the two images. He was led to this opinion by the curious facts alluded to in § 14. It would be difficult to disprove this conjecture by experiment; but all that the experiments adduced in its favour, and others relating to the disappearance of objects to one eye really proves, is, that the mind is inattentive to impressions made on one retina when it cannot combine the impressions on the two retinæ together so as to resemble the perception of some external objects; but they afford no ground whatever for supposing that the mind cannot under any circumstances attend to impressions made simultaneously on points of the two retinæ, when they harmonize with each other in suggesting to the mind the same idea.

A perfectly original theory has been recently advanced by M. Lehot†, who has endeavoured to prove, that instead of pictures on the retinæ, images of three dimensions are formed in the vitreous humour which we perceive by means of nervous filaments extended thence from the retina. This theory would account for the single appearance to both eyes of objects in relief, but it would be quite insufficient to explain why we perceive an object of three dimensions when two pictures of it are presented to the eyes; according to it, also, no difference should be perceived in the relief of objects when seen by one or both eyes, which is contrary to what really happens. The proofs, besides, that we perceive external objects by means of pictures on the retinæ are so numerous and convincing, that a contrary conjecture cannot be entertained for a moment. On this account it will suffice merely to mention two other theories which place the seat of vision in the vitreous humour. Vallee‡, without denying the existence of pictures on the retina, has advocated that we see the relief of objects by means of anterior foci on the hyaloid membrane; and Raspail§ has developed at considerable length the strange hypothesis, that images are neither formed in the vitreous humour nor painted on the retina, but are immediately perceived at the focus of the lenticular system of which the eye is formed.

§ 16.

It now remains to examine *why* two dissimilar pictures projected on the two retinæ give rise to the perception of an object in relief. I will not attempt at present to give the complete solution of this question, which is far from being so easy as at a first glance it may appear to be, and is indeed one of great complexity. I shall in this place merely consider the most obvious explanations which might be offered, and show their insufficiency to explain the whole of the phenomena.

* Act. Par. 1743. M. p. 334.

† Nouvelle Théorie de la Vision, Par. 1823.

‡ Traité de la Science du Dessein, Par. 1821, p. 270.

§ Nouveau Système de Chimie Organique, t. 2. p. 329.

It may be supposed, that we see but one point of a field of view distinctly at the same instant, the one namely to which the optic axes are directed, while all other points are seen so indistinctly, that the mind does not recognize them to be either single or double, and that the figure is appreciated by successively directing the point of convergence of the optic axes successively to a sufficient number of its points to enable us to judge accurately of its form.

That there is a degree of indistinctness in those parts of the field of view to which the eyes are not immediately directed, and which increases with the distance from that point, cannot be doubted, and it is also true that the objects thus obscurely seen are frequently doubled. In ordinary vision, it may be said, this indistinctness and duplicity is not attended to, because the eyes shifting continually from point to point, every part of the object is successively rendered distinct; and the perception of the object is not the consequence of a single glance, during which only a small part of it is seen distinctly; but is formed from a comparison of all the pictures successively seen while the eyes were changing from one point of the object to another.

All this is in some degree true; but were it entirely so, no appearance of relief should present itself when the eyes remain intently fixed on one point of a binocular image in the stereoscope. But on performing the experiment carefully, it will be found, provided the pictures do not extend too far beyond the centres of distinct vision, that the image is still seen single and in relief when this condition is fulfilled. Were the theory of corresponding points true, the appearance should be that of the superposition of the two drawings, to which however it has not the slightest similitude. The following experiments are equally decisive against this theory.

Exp. 1. Draw two lines about two inches long and inclined towards each other, as in fig. 10., on a sheet of paper, and having caused them to coincide by converging the optic axes to a point nearer than the paper, look intently on the upper end of the resultant line, without allowing the eyes to wander from it for a moment. The entire line will appear single and in its proper relief, and a pin or a piece of straight wire may without the least difficulty be made to coincide exactly in position with it; or, if while the optic axes continue to be directed to the upper and nearer end, the point of a pin be made to coincide with the lower and further end or with any intermediate point of the resultant line, the coincidence will remain exactly the same when the optic axes are moved and meet there. The eyes sometimes become fatigued, which causes the line to appear double at those parts to which the optic axes are not fixed, but in such case all appearance of relief vanishes. The same experiment may be tried with more complex figures, but the pictures should not extend too far beyond the centres of the retinæ.

Another and a beautiful proof that the appearance of relief in binocular vision is an effect independent of the motions of the eyes, may be obtained by impressing on the retinæ ocular spectra of the component figures. For this purpose the drawings should be formed of broad coloured lines on a ground of the complementary colour,

for instance red lines on a green ground, and be viewed either in the stereoscope or in the apparatus, fig. 6., as the ordinary figures are, taking care however to fix the eyes only to a single point of the compound figure; the drawings must be strongly illuminated, and after a sufficient time has elapsed to impress the spectra on the retinæ, the eyes must be carefully covered to exclude all external light. A spectrum of the object in relief will then appear before the closed eyes. It is well known, that a spectrum impressed on a single eye and seen in the dark, frequently alternately appears and disappears: these alternations do not correspond in the spectra impressed on the two retinæ, and hence a curious effect arises; sometimes the right eye spectrum will be seen alone, sometimes that of the left eye, and at those moments when the two appear together, the binocular spectrum will present itself in bold relief. As in this case the pictures cannot shift their places on the retinæ in whatever manner the eyes be moved about, the optic axes can during the experiment only correspond with a single point of each.

When an object, or a part of an object, thus appears in relief while the optic axes are directed to a single binocular point, it is easy to see that each point of the figure that appears single is seen at the intersection of the two lines of visible direction in which it is seen by each eye separately, whether these lines of visible direction terminate at corresponding points of the two retinæ or not.

But if we were to infer the converse of this, viz. that every point of an object in relief is seen by a single glance at the intersection of the lines of visible direction in which it is seen by each eye singly, we should be in error. On this supposition, objects before or beyond the intersection of the optic axes should never appear double, and we have abundant evidence that they do. The determination of the points which shall appear single seems to depend in no small degree on previous knowledge of the form we are regarding. No doubt, some law or rule of vision may be discovered which shall include all the circumstances under which single vision by means of non-corresponding points occurs and is limited. I have made numerous experiments for the purpose of attaining this end, and have ascertained some of the conditions on which single and double vision depend, the consideration of which however must at present be deferred.

Sufficient, however, has been shown to prove that the laws of binocular visible direction hitherto laid down are too restricted to be true. The law of Aguilonius assumes that objects in the plane of the horopter are alone seen single; and the law of corresponding points carried to its necessary consequences, though these consequences were unforeseen by its first advocates, many of whom thought that it was consistent with the law of Aguilonius, leads to the conclusion, that no object appears single unless it is seen in a circle passing through the centres of visible direction in each eye and the point of convergence of the optic axes. Both of these are inconsistent with the single vision of objects whose points lie out of the plane in one case and the circle in the other; and that objects do appear single under circumstances

that cannot be explained by these laws, has, I think, been placed beyond doubt by the experiments I have brought forward. Should it be hereafter proved, that all points in the plane or in the circle above mentioned are seen single, and from the great indistinctness of lateral images it will be difficult to give this proof, the law must be qualified by the admission, that points out of them do not always appear double.

XIX. *An Experimental Inquiry into the influence of Nitrogen on the growth of Plants. By* Robert Rigg. *Communicated by the Rev.* J. B. Reade, *M.A. F.R.S.*

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About two years ago I had the honour of laying before the Royal Society an experimental inquiry into some of the chemical changes which occur during the germination of seeds and the decomposition of vegetable matter. On the present occasion I purpose to confine myself to an extensive series of experiments which have reference to the presence of nitrogen, earths, and salts in vegetable compounds, with a view of directing attention to the influence of nitrogen in the growth of vegetables.

As my inquiry is purely experimental, I may premise that I have had recourse to the well-known method of ultimate analysis, and the equivalent numbers which I employ are, carbon 6·12, hydrogen 1·0, oxygen 8·0, and nitrogen 14·0. That we may the more readily apply the proportionate quantity of nitrogen to our immediate purpose, I shall make one column in each analysis, which will represent by weight the quantity of nitrogen when compared with 1000 parts of carbon in the same compound. I also designate by the term *residual* those earthy and saline ingredients which are not decomposed during the analysis. In some of the experiments this residual may contain a little *foreign matter*, for in preparing the different compounds for analysis I seldom had recourse to any process of ablution, rather choosing to have a little foreign matter present, than to remove any part of that which was more particularly the object of research. That I might also examine the compounds as nearly as possible in their natural state, I very rarely exposed them to a higher temperature than 100° Fahr., inclosing them in very thin paper, and afterwards allowing them to acquire the hygrometric state of the atmosphere.

The first series of experiments to which I shall refer tends to show, that in that part of the seed where germination takes place nitrogen preponderates, when compared with its quantity in the other part of the seed. This result is derived from the analysis of the germ and cotyledons of beans, peas, barley, wheat, &c., a large excess of nitrogen being invariably indicated in the germ.

Table I.

	Carb.	Hydr.	Oxygen.	Nitr.	Resid.	Water.	Total.	Nitr. for 1000 Carb.
Germ of the garden bean	42·68	1·19	....	8·53	1·80	45·8	= 100	200
Cotyledons of the garden bean......	39·27	....	2·66	5·65	2·40	50·02	= 100	140
Germ of early garden peas	41·9	0·2	....	8·3	0·8	48·8	= 100	198
Cotyledons of early garden peas	40·1	....	6·5	4·2	1·3	47·9	= 100	104
Germinating ends of barley	39·6	0·2	....	1·9	0·6	57·7	= 100	48
The other parts of barley	39·2	....	....	1·0	0·8	59·0	= 100	25
Germinating ends of wheat	41·2	0·9	....	2·1	0·7	55·1	= 100	51
The other parts of wheat	40·6	0·3	....	1·6	0·8	56·7	= 100	39

Thus, for instance, it appears from the table of analysis, that the germ of beans and peas contain by weight about 200 parts of nitrogen for 1000 parts of carbon, while the cotyledons contain only from about 100 to 140 parts.

A second series of experiments disposes me to think, that those *seeds of the same kind* which contain the largest quantity of nitrogen germinate the earliest. Barley of the growth of 1835, containing 46 parts of nitrogen for 1000 of carbon, germinated in thirty-six hours after being taken out of the water in which it had been steeped; whereas barley of 1837, and containing only 35 parts of nitrogen, steeped in water at the same time, and kept under the same circumstances as the former, germinated in forty-eight hours. Similarly, I find that of the seeds, mustard, cress, rape, turnip, radish, and celery, those which contain the largest quantity of nitrogen and residual, germinate the earliest when kept under equal circumstances. It is necessary to state, that in these analyses the seeds were examined in the mass.

Table II.

	Carb.	Hydr.	Oxygen.	Nitr.	Resid.	Water. Total.	Nitr. for 1000 Carb.
Barley of 1835	43·93	0·71	....	2·02	1·30	52·04 = 100	46
Barley of 1837	39·57	....	3·45	1·38	1·30	54·30 = 100	35
Mustard seed	50·74	2·36	....	3·55	3·90	39·45 = 100	70
Cress seed	46·8	1·5	....	3·3	4·8	43·6 = 100	71
Rape seed	55·3	3·4	....	2·7	3·1	35·5 = 100	50
Turnip seed	55·4	3·5	....	3·6	3·1	34·4 = 100	65
Radish	55·34	3·48	....	5·03	3·4	32·75 = 100	90
Celery....................	50·39	2·35	....	2·37	6·6	38·29 = 100	47

The chemical constitution of the *rootlets of seeds* before the *plumula* extends the whole length of the seeds, as in the instance of malted barley, differs from that of the malt, and also from the constitution of the barley in its original state. In these we have the rootlets containing a large quantity of nitrogen at a period when they will have to perform important offices in preparing the food for the young plant. That there is a similar difference between the chemical constitution of the roots and trunks of trees will abundantly appear from the annexed Table. And I may also add, that my experiments dispose me to infer that the quantity of nitrogen is largest in the spring, and diminishes with the season.

Table III.

	Carb.	Hydr.	Oxygen.	Nitr.	Resid.	Water. Total.	Nitr. for 1000 Carb.
Barley in its original state..........	39·6	....	3·5	1·3	1·3	54·3 = 100	32
The malt made from the same	41·7	....	1·8	2·1	1·4	53·0 = 100	50
The rootlets of the malted barley....	40·5	0·4	....	4·3	3·5	51·3 = 100	106
Root of an apple-tree with the bark..	41·6	....	3·7	·7	1·2	52·8 = 100	16
Trunk of an apple-tree without the bark	42·8	....	4·8	·3	·3	51·8 = 100	7
Root of a plum-tree with the bark ..	42·4	0·4	....	·8	1·6	54·8 = 100	18
Trunk of the same without the bark..	42·8	....	....	·4	0·6	56·2 = 100	9
Root of cherry-tree with the bark ..	45·5	0·2	....	1·1	·8	52·4 = 100	18
Trunk of cherry-tree without the bark	43·9	....	4·3	·6	·2	51·0 = 100	10
Ash in a very dry state without the bark	46·6	....	....	0·5	·8	52·1 = 100	11
Ash root without the bark	35·2	0·8	....	2·6	9·4	52·0 = 100	74

Note.—The apple, plum, and cherry-trees were all of them very small; they had been in the ground several years, and had been rooted up because of their general unhealthiness. In a healthy state of the trees the nitrogen of the root is in a larger proportion.

But not only is the nitrogen more abundant in the roots of plants and trees; the residual also, when compared with the quantity in the trunks, will be found in excess in the roots.

Now if we admit the principle, that nitrogen is a powerful agent in favouring chemical action upon vegetable and animal matter, and that this residual is essential to the healthy performance of every function of the roots, as well as every other part of the plant, and forms, as it were, a most perfect skeleton of the whole; we have in these roots that which will favour such action in an eminent degree when compared with the other part of the tree.

It would be leading us into other subjects more extensive than the one now before us, if I were to go into, or treat upon, the chemical action which takes place by the agency of the roots, the compounds formed thereby, the heat produced by such action, the arrangement of the residual, &c. It will be sufficient, that in following up this part of the inquiry, we state as the result of experiment, that the *sap wood* is very differently constituted from the more perfect part, *the heart wood*, an excess of nitrogen being invariably found in the former.

Table IV.

	Carb.	Hydr.	Oxygen.	Nitr.	Resid.	Water. Total.	Nitr. for 1000 Carb.
Young English oak sap wood	42·40	·18	....	·55	1·6	55·27 = 100	13
Heart wood of ditto	42·40	....	....	·16	0·7	56·74 = 100	4
Quebec oak sap wood	44·2	·14	....	·31	1·3	54·05 = 100	7
Heart wood of ditto	44·59	....	....	·13	0·6	54·68 = 100	3
English elm sap wood	41·20	·40	....	1·6	2·5	54·3 = 100	39
Heart wood of ditto	41·1	....	....	0·7	1·7	56·5 = 100	17
Acacia sap wood	43·79	....	2·49	·55	·50	52·67 = 100	13
Heart wood of ditto	41·36	....	6·67	·50	·20	51·27 = 100	12
Cedar from Africa sap wood	42·06	·42	....	·39	2·8	54·33 = 100	10
Heart wood of ditto	39·92	....	....	·36	0·7	59·02 = 100	9
Chestnut sap wood	41·16	....	1·86	·38	·40	56·20 = 100	9
Heart wood of ditto	40·18	....	7·96	·29	·20	51·37 = 100	7

It will be unnecessary for me to say that the sap wood more readily passes into a state of decay than the heart wood. Here again the nitrogen and the residual being present in larger quantities in the former than in the latter, we have them exerting their influence as promoters of decomposition.

We have also the greatest quantities of nitrogen and residual in those timbers which grow the quickest: and further than this; for directly as the quantity of nitrogen and residual taken collectively, so do we appear to have the decay of timber, all other circumstances being equal. The following is the analysis of several kinds of timber which favour this inference.

TABLE V.

	Carb.	Hydr.	Oxygen.	Nitr.	Resid.	Water. Total.	Nitr. for 1000 Carb.
Satin wood	47·20	....	....	·04	·50	52·26 = 100	1
Dantzic oak	44·90	....	....	·11	·50	54·49 = 100	2
English oak	43·20	....	....	·20	·60	56·00 = 100	4
Malabar teak	46·82	·48	....	·26	·18	52·26 = 100	5
Rangoon teak	47·93	·34	....	·43	·22	51·08 = 100	9
Spanish mahogany, fine-grained	49·0	....	....	·10	·50	50·4 = 100	2
Honduras mahogany, coarse-grained	40·97	....	....	·90	1·50	56·63 = 100	23
Lignum vitæ	51·22	1·21	....	·56	·60	46·41 = 100	11
Box	46·4	·50	....	·50	·80	51·80 = 100	11
Rose wood	51·1	....	....	·60	2·50	45·8 = 100	12
Black ebony	42·4	....	....	1·50	5·0	51·1 = 100	35
American birch	45·0	....	....	2·2	1·5	51·3 = 100	40

Thus, for instance, the nitrogen in the satin wood may be considered almost inappreciable; and the same may be said of the residual in the Malabar teak, the nitrogen being also small in this timber. In Dantzic and English oak the quantity of nitrogen and residual are both very small. In American birch the nitrogen and residual are in large quantities, and, as is well known, this timber decays very quickly.

But it is not enough for us to find a difference in the proportionate quantity of nitrogen in the different parts of the same plant or tree; we must also observe that the quantity appears to be proportional to the functions which the parts of the plants have to perform in vegetation. For instance, if the agency of any part of the plant be great in the scale of vegetable physiology, so is the quantity of nitrogen, and *vice versâ*. So apparent is this, and so universal is the operation of this law over the whole sphere of inquiry in which I have been engaged, that we might almost consider this element, when coupled with the residual, to be the moving agent, acting under the influence of the living principle of the plant, and moulding into shape the other elements. We have this beautifully instanced in the chemical constitution of the different parts of wheat, barley, oats, common grass, turnips, cabbages, carrots, potatoes, &c., found by subjecting their various parts to analysis *at different periods of their growth* (See Table VI.). For by thus subjecting the different parts of the same plant to analysis at different periods of growth, we acquire much valuable information upon vegetation generally, and respecting the influence of nitrogen and residual in particular.

Table VI.

	Carbon.	Hydr.	Oxygen.	Nitr.	Resid.	Water.		Total.	Nitr. for 1000 Carb.
Flour of wheat not nearly ripe	41·2	1·8	....	2·9	2·0	52·1	=	100	70
Flour of the same kind nearly ripe ..	40·6	0·5	....	2·3	1·0	55·6	=	100	57
Leaves of the wheat not nearly ripe..	37·6	....	8·1	3·3	4·2	46·8	=	100	87
Leaves of the same when nearly ripe	38·4	....	....	2·1	4·6	54·9	=	100	55
Stems of the wheat not nearly ripe ..	39·8	0·8	....	3·5	4·0	51·9	=	100	87
Stems of the same when nearly ripe..	38·8	....	....	1·3	4·0	55·9	=	100	33
Chaff of wheat not nearly ripe	35·5	....	7·3	1·8	10·8	44·6	=	100	50
Chaff of the same when nearly ripe ..	31·2	....	1·7	1·3	11·0	54·8	=	100	42
Common grass not growing freely ..	41·1	....	3·1	4·4	5·5	45·9	=	100	107
Common grass gathered at the same time, growing very freely	39·5	1·6	....	5·6	6·5	46·8	=	100	141
Turnip when attacked by the fly	35·5	....	....	8·0	13·4	43·1	=	100	224
Cabbage leaf not eaten	39·5	....	4·8	8·1	5·9	41·7	=	100	203
The part eaten by insects	39·7	....	13·8	5·7	8·0	32·8	=	100	143
The insects themselves	36·0	1·3	....	6·3	14·0	42·4	=	100	175
Green part of another cabbage leaf ..	39·9	....	3·8	6·5	4·7	45·1	=	100	162
White part of the same............	39·2	0·8	....	8·0	4·9	47·1	=	100	205
Tendril of the same	38·8	2·7	....	5·4	6·3	46·8	=	100	138
Very centre part of the cabbage	33·0	1·7	....	4·1	4·0	57·2	=	100	124
Root of the same plant	39·2	1·4	....	5·5	4·5	49·4	=	100	141
Red clover stems	29·6	0·4	....	2·5	9·8	57·7	=	100	83
Leaf	28·6	....	7·7	4·2	5·0	54·5	=	100	145
Flower	30·4	....	10·2	3·6	5·0	50·8	=	100	119
Potato itself	37·1	1·4	....	2·9	3·4	55·2	=	100	79
Stem of the same	25·3	....	18·1	3·1	15·0	38·5	=	100	123
Leaves of the same	39·8	0·5	....	8·5	9·4	41·8	=	100	214
Apple of the same	32·9	....	16·4	3·9	5·6	41·2	=	100	117
Corolla of the same	38·8	....	8·5	3·3	4·4	45·0	=	100	85
Pistils of the same................	36·2	2·2	....	4·6	9·6	47·4	=	100	129
Young carrot, $\frac{1}{4}$ of an inch in diameter	33·1	1·5	....	2·9	8·5	54·0	=	100	88
Leaves of the same	30·4	0·8	....	2·7	10·0	56·1	=	100	90
Stems of the same	28·7	....	2·8	1·7	11·2	55·6	=	100	59

There appear indeed to be various chemical actions taking place, in which these two elements are eminently concerned, viz. in the preparation of the food of the plants by the roots, and in combining this food with the other elements and fitting the whole to the various purposes of the plants.

Throughout the whole course of my experimental inquiry, I have not met with one instance wherein we have a large proportion of nitrogen and residual, that we have not violent chemical action and quick growth of the plants, all other circumstances being favourable.

By analysing *the leaves of trees* we may throw further light upon the operation of nitrogen. Of the almost numberless vegetables which cover the face of the earth, there are very few, if any, whose growth and produce afford us more information upon the chemical changes which occur during the growth of plants and the decomposition of vegetable matter than the vine. Its abundant flow of sap in the spring yields us a most important product for determining its food. Its foliage furnishes us with a plentiful supply of leaves for examination at different seasons: and by allow-

ing these leaves sometimes to remain on the trees until they are very abundant, and then removing a considerable portion thereof, leaving the rest to grow, we have at intervals of very few days an opportunity of chemically examining this very important and indispensable part of vegetable production under very different circumstances. By carefully dissecting these leaves, we are enabled to discover by analysis important changes produced in very few hours. From the proneness on the part of these leaves to pass into decomposition, at favourable temperatures, we have a feature brought before us which claims our best attention. And we have the fruit of this plant affording us, in its conversion into wine and other substances, an opportunity of examining into many important chemical changes, and I may add, of making the accuracy of many popular theories more than questionable.

The vines which more generally afforded me materials for examination are those which produce the white and black sweet-water grapes. They are in the open air, and are nailed to the south side of a brick wall. A series of experiments upon the leaves of these vines are given in Table VII., showing in a striking manner that nitrogen is in large quantities when they first make their appearance; that as they are developed, it decreases in proportionate quantity; that it is in excess during the period of their most rapid growth; and that towards the close of the year it is comparatively small.

Table VII.

	Carbon.	Hydr.	Oxygen.	Nitr.	Resid.	Water.	Total.	Nitr. for 1000 Carb.
The first small leaves on the white grape vine	37·7	0·5	....	5·1	9·5	47·2	= 100	134
Leaves from the same about a month afterwards	42·2	0·4	....	5·3	3·8	48·3	= 100	126
Leaves from the same in July	39·8	....	4·2	3·5	3·8	48·7	= 100	88
Leaves from the same in August	39·1	....	6·1	2·9	6·6	45·3	= 100	74
Leaves from the same in November	41·9	....	....	2·3	9·2	46·6	= 100	55
The first leaves on the black grape vine	41·8	1·4	....	7·8	10·3	38·7	= 100	185
Leaves from the same in June	42·8	....	3·8	5·4	3·8	44·2	= 100	126
Leaves from the same in July	41·5	....	1·1	3·6	3·0	50·8	= 100	88

With a view of ascertaining whether or not these peculiarities in the chemical constitution of the leaves of plants and trees were universal, I have had recourse to extensive analyses thereof, gathering the leaves from a great number of trees at different stages of their growth. The results hereby furnished may be obtained from the experiments in Table VIII.

TABLE VIII.

	Carbon.	Hydr.	Oxygen.	Nitr.	Resid.	Water. Total.	Nitr. for 1000 Carb.
First small leaves from the lime tree in May	41·9	1·0	....	7·1	6·5	43·5 = 100	169
From the same in September 17	34·3	....	11·3	3·6	5·4	45·4 = 100	105
From the same in October 12	33·8	....	3·4	2·9	5·6	54·3 = 100	76
Acacia leaves, August 26, 1836	43·3	0·4	....	6·2	4·4	45·7 = 100	144
Acacia leaves, October 20, 1836	39·8	....	....	3·3	6·4	50·5 = 100	82
Almond leaves, August 26, 1836........	37·5	....	11·7	4·4	3·8	42·6 = 100	118
Almond leaves, September 27	37·0	....	....	3·8	4·0	55·2 = 100	102
Plane tree leaves, September 26, 1836....	41·4	....	18·7	2·7	4·4	32·8 = 100	65
Plane tree leaves, October 26	45·3	....	....	2·4	3·8	48·5 = 100	53
Red currant, May 3	40·1	....	2·0	6·2	3·5	48·2 = 100	155
Red currant, August 25	44·6	....	....	5·7	3·6	46·1 = 100	129
Very young ivy	40·4	1·6	....	3·6	4·6	49·8 = 100	90
Full grown ivy	41·6	0·4	....	3·2	5·2	49·6 = 100	78
Decaying ivy	42·4	....	....	2·2	5·8	49·6 = 100	52
Oak leaves, July 1836	40·8	0·6	....	4·3	3·9	50·4 = 100	104
Oak leaves, August	38·4	....	....	3·8	4·0	53·8 = 100	100

The analyses of the different parts of the flowers of plants are full of interest. The parts not only differ in chemical constitution with their state of developement, as appears in Table IX., in the instance of the rose, where the full-blown petals contained twenty-four parts of nitrogen, and the unexpanded and central petals contained sixty-six parts; but the various portions differ very materially from each other, and when taken in connexion with the germination of seeds, the growth of plants, their aliment, &c., throw much light upon the whole subject.

TABLE IX.

	Carbon.	Hydr.	Oxygen.	Nitr.	Resid.	Water. Total.	Nitr. for 1000 Carb.
Full-blown rose petals	42·2	....	2·6	1·0	3·0	51·2 = 100	24
Rose petals not expanded, gathered at the same time, and from the same tree	45·2	1·1	....	3·0	2·5	48·2 = 100	66
Petals of the dahlia	35·5	....	10·2	2·7	4·0	47·6 = 100	77
Pistils of the same	34·8	1·3	....	4·2	4·3	55·4 = 100	120
Petals of the white lily	36·4	....	13·5	1·9	5·2	43·0 = 100	53
Pistils of the white lily	38·1	0·3	....	3·6	4·5	53·5 = 100	94
Pollen of the white lily	55·4	5·5	....	5·6	5·8	27·7 = 100	101
Stems of the anthers of white lily ..	40·5	....	....	2·2	5·0	52·3 = 100	55
Chrysanthemum, expanded petals....	39·2	....	....	2·1	3·7	55·0 = 100	54
Chrysanthemum, unexpanded	39·2	3·0	....	2·9	2·4	52·5 = 100	74
Pollen of the same	43·2	1·6	....	3·0	1·8	50·4 = 100	69
Leaves of the same................	40·2	0·6	....	2·8	8·2	48·2 = 100	70
Leaves gathered June 16	41·4	....	4·1	5·0	4·2	45·3 = 100	121

Without adding to the number of experiments already furnished, I would observe, that I have not analysed any product in a natural state wherein I have not found both nitrogen and residual; and, of the great number that I have subjected to this

process, those which are embodied in this paper may be considered as approximating to an average of the whole, as regards both this gaseous element and the incombustible matter.

In conclusion, I would observe that the mode of analysis which I have adopted in the examination of organic compounds, so far as determining the quantity of carbon, hydrogen, oxygen, and residual are concerned, is the one described in the paper on vegetable decomposition to which I have already referred. Respecting the mode of determining the quantity of nitrogen, a very brief account of the plan which I have adopted is given in the Philosophical Magazine for January last; and by combining these two methods of ultimate analysis, I am enabled, in recapitulation, to detect very minute errors, and therefore to speak with certainty as to the accuracy and value of every experiment.

XX. *On the Evolution of Nitrogen during the growth of Plants, and the Sources from whence they derive that element. By* Robert Rigg. *Communicated by the Rev.* J. B. Reade, *M.A. F.R.S.*

Received June 14,—Read June 21, 1838.

In this communication I shall have the honour of submitting to the Royal Society a series of experiments, which not only confirm the principles I have already laid down in the investigations of the influence of nitrogen on the growth of plants, but also enable us to trace this important element to its source.

By what mysterious process of natural chemistry the living principle of the plant obtains this product, we must be content to remain ignorant; but to what extent a supply can be procured from the compounds upon which its agency is directed, is within the compass of legitimate experimental research.

With respect to the entire volume of nitrogen connected with the process of vegetation, it will be observed that my former Tables show that the quantity appropriated by the plant varies from $\frac{1}{1000}$ to $\frac{1}{5}$ of their weight of carbon; and the important experiments of Dr. Daubeny, Saussure, Sir Humphry Davy and others, have abundantly established the fact that there is a considerable evolution of nitrogen during the growth of plants. Experiments, however, upon the quantity of nitrogen evolved are so beset with difficulties, that our best efforts in this department of vegetable physiology have not enabled us to speak with certainty as to the absolute or relative quantities of the gases which are given off during the healthy action of the functions of vegetable life. And in corroboration of this remark it is only necessary to state, that the natural course of vegetation is unavoidably interrupted, by the necessity we are under of excluding plants from the free action of the external atmosphere, when we attempt to collect the gases they give off.

Sir Humphry Davy made his experiments under as favourable circumstances as the nature of the case will admit of; and he found that when the oxygen decreased in quantity about two per cent. the nitrogen increased four per cent., and that when the oxygen increased about thirty-five per cent. the nitrogen increased about seven per cent. Some of my own experiments have been conducted on the principle adopted by Sir Humphry Davy. I have also introduced whole branches of trees, as well as stems and leaves of more tender plants, under water, without removing them from the parent stock, and collected their gaseous products in inverted glass vessels. Upon examining these products, I find, that the gaseous volumes are perceptibly affected by the brightness of the sun's rays, though their variation appears to be subject to no

fixed law. At the same time, however, it is certain that nitrogen, whether appropriated or evolved, is apparently the most uniform, while oxygen assumes the character of a most fluctuating and passive agent.

Hence, then, if plants, taken as a whole, contain from three to four per cent. of their weight of nitrogen, and if to this we add the indefinite quantity, so far as we can comprehend, which is evolved during their growth, the question naturally arises, From whence do plants draw this part of their substance, as well as the volume of nitrogen which they furnish to the atmosphere? a question of the greatest importance in the practical application of the subject, and one not wanting in interest as a part of the inquiry into the chemical changes which occur during the growth of plants.

The quantity of animal and vegetable matter which forms a part of all soils suitable for the growth of plants, invariably containing nitrogen, furnishes us at once with one source from whence they may derive a part, if not the whole, of this element, which is either found in their constitution, or given off to the atmosphere during their growth. Hence an accurate examination of these soils at different periods might enable us to determine the question as to whether or not the earth supplied the whole of the nitrogen which is employed in vegetable economy. But from the many difficulties consequent upon accurately experimenting upon soils previous to and after the production of any plants, and also the examination of the plants so produced, we are not able by this mode of proceeding to obtain results in any degree satisfactory.

In order to free myself from all doubt upon this point, and to ascertain whether *the atmosphere* did or did not furnish any part, and if any what proportion, of the nitrogen which enters into the constitution of plants, I had recourse to the ultimate analysis of seeds and young seedling plants.

Some seeds I steeped in distilled water, and some in filtered Thames river water. When they had absorbed a sufficient quantity of water to favour the first impulse of germination, I spread them on glass or china plates, and kept them at temperatures favourable to germination and vegetation, in a room where the only source of nitrogen would be that which was supplied by the seed and the atmosphere. The quantity of dry seed experimented upon varied from thirty to one thousand grains, which were accurately weighed. These I allowed to germinate and grow, keeping them regularly supplied with the respective kinds of water only; and that nothing might be lost by the water carrying off any of the soluble parts of the different seeds, that water which was employed in steeping each kind of seed at the commencement was afterwards used in watering the same.

Some of the experiments I favoured so as to have them germinating and growing quickly, and others slowly. Some of them were terminated at one stage of their growth, and some at another; and some of them were allowed to vegetate as long as the seeds appeared to afford them nourishment. At the end of each experiment the whole mass was enveloped in very thin paper, and dried at a temperature varying from 100° to 110° Fahr., powdered, and afterwards allowed to acquire the hygrometric state

of the atmosphere. They were accurately weighed in this condition, a portion of each part, or of the entire mass, subjected to ultimate analysis, and calculation made for the whole. Thus I was enabled to discover an increase or decrease of any of the elements in any experiment so conducted, wherein the seed itself, the water, and the atmosphere furnished whatever was required for vegetation. The experiments which have reference to this part of the subject are found in Table X.

We are led by these experiments to the inference, that, independent of that indefinite quantity of nitrogen which is given off to the atmosphere during vegetation, there is an increase of this element in plants when compared with its quantity in the seeds; and in this case the seeds form the only source from whence they could derive it, with the exception of the atmosphere and any little which might have combined with the water used on the occasion.

It would be at variance with my mode of research, which is purely experimental, to make any observations upon the quantity of nitrogen which is probably furnished by the atmosphere during the germination of seeds and the full growth and developement of plants. The experiments before us dispose us to infer that it differs with the temperature at which the plants are exposed, and with exposure as regards sunshine and the shade. Thus we have in the germination and vegetation of barley, for instance, the quantity of nitrogen in the grain germinated under very favourable circumstances for the process, increasing to an extent equal to thirty-eight per cent. upon the original quantity contained in the seed: when the same kind of barley was kept under unfavourable circumstances for vegetation, and allowed to grow until the principal part of the farina was exhausted, the increase in the quantity of nitrogen was only eighteen per cent. When the same grain was allowed to vegetate in the sun's rays until about two-thirds of the flour contained in the seed had disappeared, the increase in the quantity of nitrogen was thirty per cent.; and when the same plants were kept under the most favourable circumstances, and allowed to vegetate until the seeds appeared to be exhausted, the young plant during this time having the most healthy appearance, there was an increase upon the quantity of nitrogen contained in the seeds of *nearly fifty per cent.*

Seeds of cress during vegetation increased their quantity of nitrogen forty-one per cent. when the plants were kept under a temperature varying with shade and sunshine from 60° to 84°. The experiments upon the seeds of the turnip, an important plant in an agricultural point of view, (about one half of which germinated,) shows that the nitrogen which was derived from the atmosphere was more than that which was contained in the seed. In all these experiments we have the quickness of the growth of the young plants proportional to the quantity of nitrogen present when compared with 1000 parts of carbon in the same.

With these facts before us, we are enabled to account for plants not continuing to grow so well in pure oxygen gas as in atmospheric air; and by following up the inquiry in other departments, we see the wisdom of the all-wise Contriver in consti-

tuting an atmosphere with a decided preponderance of nitrogen and a much smaller proportion of oxygen.

Finding that plants under different circumstances of growth differ in the relative quantity of nitrogen which they contain, as well as in the other elements which enter into their constitution, I was desirous of obtaining information which would lead us to account for the well-known fact of plants increasing most in size during cloudy weather, and of grasses, for instance, which are shaded (as under hedges), increasing much in straw, but producing seed both small in quantity and inferior in quality.

By way of commencement in this part of the research, I made duplicate experiments with the same kind of seeds, steeped them in and supplied them with the same kind of water, and kept them under equal circumstances in every respect, except that of placing one of each in the sun's rays in the greenhouse, where the sun shone till three P.M., and the other in the same situation, excluded however from the sun's rays, but not from the light. Those in the shade increased in length much more than the others, were a little lighter in colour, and when weighed before they were exposed to the drying temperature, were also heavier, but when dried at 100° to 110° were considerably less in weight. The ultimate analyses of these experiments are found in Table XI., and the result of the experiments as regards nitrogen is, that those plants which weighed the heaviest before drying, and which had as it were moulded into shape the largest quantity of matter in the form of plants, contained, in the cress for instance, 147 parts of nitrogen for every 1000 parts of carbon; whereas those plants from seed of the same kind which grew in the sun's rays, and whose weight before drying and when freed from foreign water was nine per cent. lighter than the other, contained only 111 parts of nitrogen for 1000 of carbon;—and in addition I might make an observation which is in perfect harmony with all that has been noticed upon the influence of nitrogen on the growth of plants, viz. that whereas the cress arrived at its state of maturity, so far as the seed could furnish it with nourishment, in eighteen days, the rape had not exhausted all its seed in twenty-six days; and the proportionate quantity of nitrogen in those plants was, in cress in the sun's rays 111, in rape in the same situation 73; and in cress in the shade 147, and in rape 82, when compared with 1000 parts by weight of carbon in each.

In concluding this subject, upon which I have been as brief as its nature would admit of, not even entering at all upon the practical application thereof, the point of view wherein its real value consists, I beg to observe that, although nitrogen appears from these experiments to be a very powerful agent in the economy of plants, it is far from my intention to give it any undue importance. It is my object to draw attention to an element which, comparatively speaking, has escaped unnoticed, and to vindicate the necessity of a most scrupulous attention to those products which, though so minute in quantity as to be with difficulty detected in our balances, have nevertheless been wisely assigned to discharge the most important functions.

Table X.

		Carb.	Hydr.	Oxyg.	Nitr.	Resid.	Water. Total.	Nitr. for 1000 Carb.
Barley steeped in distilled water germinated quickly until the plumula of several seeds had passed through the grain.	The malt	35·14	....	1·51	1·73	1·15	44·67 = 84·2	49
	The rootlets and plumula	1·70	0·2	....	·18	·16	2·14 = 4·2	106
		36·84	0·2	1·51	1·91	1·31	46·58 = 88·4	
Barley in its original state		39·57	....	3·45	1·38	1·30	54·3 = 100	35
Increase		....	0·2	....	·53	·01		
Decrease		2·73	....	1·94	....	....	7·72 = 11·6	
Barley steeped in river water vegetated in the shade until the corn appeared to be exhausted.	The stems ..	7·98	·17	....	·61	·86	11·38 = 21·0	76
	The roots....	8·90	....	....	·72	1·00	13·37 = 23·99	81
	The husks ..	6·19	....	·25	·26	·53	7·27 = 14·5	24
	The liquid separated by drying	·04	·01	....	·02	....	= ·07	
		23·11	·18	·25	1·61	2·39	32·02 = 59·56	
Barley in its original state		39·57	....	3·45	1·38	1·30	54·3 = 100	35
Increase		....	·18	....	·23	1·09		
Decrease		16·46	....	3·2	....	....	22·28 = 40·46	
Barley steeped in distilled water, and vegetated until about two-thirds of the flour had disappeared; kept in the sun's rays.	Original state	39·57	....	3·45	1·38	1·30	54·3 = 100	35
	The vegetated mass	29·7	·5	....	1·80	1·3	44·3 = 77·6	60
	Increase	....	·5	....	·42	....	....	25
	Decrease	9·87	....	3·45	....	....	9·7 = 22·4	
Barley steeped in rain water grew until the stems were five and six inches long; kept in the sun's rays.	Original state	39·57	....	3·45	1·38	1·30	54·3 = 100	35
	Young plants	27·83	....	4·13	2·06	1·36	33·82 = 69·2	70
	Increase	....	....	·68	·68	·06	....	35
	Decrease	11·74	....	....	....	....	20·48 = 31·8	
Cress seed supplied with distilled water, and kept in the sun's rays.	The seed	46·77	1·53	....	3·27	4·8	43·63 = 100	71
	Young plants	37·59	·12	....	4·64	4·75	46·10 = 93·2	121
	Increase	....	....	....	1·37	....	2·47	50
	Decrease	9·18	1·41	....	....	0·05	6·8	
The same seed supplied with river water, and kept more in the shade.	The seed	46·77	1·53	....	3·27	4·8	43·63 = 100	71
	Young plants	33·9	·80	....	4·1	4·9	40·9 = 84·6	121
	Increase	....	....	....	·83	·1	....	50
	Decrease	12·85	·73	....	....	....	2·73 = 15·4	
Turnip seed supplied with river water, about half of which germinated.	The seed	55·48	3·45	....	3·55	3·1	34·42 = 100	65
	After vegetat.	40·70	1·82	....	4·48	4·48	32·62 = 84·5	109
	Increase	....	....	....	·93	1·38	....	44
	Decrease	4·78	1·63	....	....	....	1·8 = 15·5	
Rape seed, about two-fifths of which grew.	The seed	55·29	3·45	....	2·71	3·1	35·45 = 100	50
	After vegetat.	44·31	1·58	....	3·14	3·1	40·37 = 92·5	73
	Increase	....	....	....	·43	....	4·92	23
	Decrease ...	10·98	1·87	....	....	....	.... 7·5	

Table XI.

		Carb.	Hydr.	Oxyg.	Nitr.	Resid.	Water.	Total.	Nitr. for 1000 Carb.
Cress seed supplied with river water, and kept in the sun's rays in the greenhouse.	The seed	46·77	1·53	....	3·27	4·8	43·63	= 100	71
	Young plants.	40·03	·98	....	4·44	10·5	36·35	= 92·3	111
Increase		....	....	....	1·17	5·7	....	....	40
Decrease		6·74	·55	....	....	....	7·28	= 7·7	
The same kind of seeds, but kept in the shade.	The seeds....	46·77	1·53	....	3·27	4·8	43·63	= 100	71
	Young plants	30·08	·92	....	4·42	9·97	33·91	= 79·3	147
Increase		....	....	....	1·15	5·17	...	....	76
Decrease		16·69	·61	....	....	....	9·72	= 20·7	
Rape seed supplied with river water, and kept in the sun's rays.	The seeds....	55·29	3·45	....	2·71	3·1	35·45	= 100	50
	Young plants	45·35	1·38	....	3·20	8·12	48·75	= 106·8	73
Increase		....	....	....	0·49	5·02	13·3	= 6·8	23
Decrease		9·94	2·07						
The same kind of seeds kept in the shade.	Seed........	55·29	3·45	....	2·71	3·1	35·45	= 100	50
	Young plants	39·34	1·90	....	3·19	7·52	38·65	= 90·6	82
Increase		....	....	....	·48	4·42	3·2	=	32
Decrease		15·95	1·55	....	....	....	....	9·4	
Mustard seed steeped in river water; kept in the sun's rays.	Seed........	50·74	2·36	....	3·55	3·9	39·45	= 100	70
	Young plants	33·33	·88	....	3·98	5·0	37·51	= 80·7	119
Increase		....	....	....	·43	1·1	....	....	49
Decrease		17·41	1·48	....	....	....	1·94	= 19·3	
The same kind of seed kept in the shade.	Seed........	50·74	2·36	....	3·55	3·9	39·45	= 100	70
	Young plants	31·68	1·66	....	3·93	3·85	30·21	= 71·33	128
Increase		....	....	....	·38	....	....	....	58
Decrease		19·06	·70	....	....	·05	9·24	= 28·67	
The chemical constitution of the young plants mentioned in Table XI. when brought to 100 parts are	Cress in sun's rays	43·36	1·06	....	4·81	11·4	39·37	= 100	111
	Cress in the shade	37·92	1·16	....	5·57	12·6	42·75	= 100	147
	Rape in sun's rays	42·46	1·29	....	3·00	7·6	45·65	= 100	73
	Rape in the shade	43·39	2·1	....	3·55	8·3	42·66	= 100	82
	Mustard in the sun's rays	41·32	1·09	....	4·93	6·2	46·46	= 100	119
	Mustard in the shade	44·41	2·32	....	5·51	5·4	42·36	= 100	128

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TO THE

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FOR THE YEAR 1838.

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OBSERVANDA.

Height of the Cistern of the Barometer above the plinth at Waterloo Bridge....83 feet 2 inches.
________________ above the mean level of the sea ....97 feet.
Height of the receiver of the Rain Gauge above the court of Somerset House ..79 feet.
The External Thermometer is 2 feet higher than the Barometer Cistern.
The Thermometers are graduated to Fahrenheit's scale.
The Barometer is divided into inches and tenths.
The Hours of Observation are of Mean Time, the day beginning at Midnight.
The *daily* observations of the Barometer are *not* corrected.
The *monthly means* are corrected for capillarity and temperature by the Table contained in Mr. Baily's paper in *Phil. Trans.* for 1837.

METEOROLOGICAL JOURNAL FOR JANUARY AND FEBRUARY, 1838.

1838.	9 o'clock, A.M. Barometer uncorrected. Flint Glass.	9 o'clock, A.M. Barometer uncorrected. Crown Glass.	9 o'clock, A.M. Att. Ther.	3 o'clock, P.M. Barometer uncorrected. Flint Glass.	3 o'clock, P.M. Barometer uncorrected. Crown Glass.	3 o'clock, P.M. Att. Ther.	Dew Point at 9 A.M., deg. Fahr.	Diff. of Wet and Dry Bulb Ther.	External Thermometers. Fahrenheit. 9 A.M.	External Thermometers. Fahrenheit. 3 P.M.	External Thermometers. Self-registering. Lowest	External Thermometers. Self-registering. Highest	Rain in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
JANUARY M 1	29.978	29.972	48.3	29.924	29.918	49.2	43	02.3	45.7	48.3	43.8	50.2		SSE	A.M. Fine—light clouds and wind. P.M. Dark heavy clouds. Evening, Overcast—very fine rain.
T 2	29.888	29.882	47.6	29.794	29.786	48.5	43	01.8	45.8	46.7	43.0	48.7		SE	A.M. Overcast—very light mist and wind. P.M. Fine—light clouds and wind. Evening, Overcast—deposition.
W 3	29.642	29.636	47.2	29.692	29.686	47.8	43	02.1	44.3	47.3	43.2	48.8		SSE	Fine—light clouds, with light brisk wind throughout the day. Evening, Fine and clear.
T 4	29.976	29.970	44.8	30.006	30.000	45.7	39	01.8	40.2	46.6	39.4	48.3		S	Fine—light clouds & wind throughout the day. Ev. Fine and clear.
F 5	30.270	30.262	44.0	30.264	30.256	43.3	38	01.2	37.4	36.7	35.8	47.4		W	Thick fog throughout the day. Evening, Foggy.
S 6	30.292	30.284	40.8	30.242	30.236	41.0	34	01.4	35.5	37.7	32.3	38.7		SW	A.M. Thick fog—deposition—light wind. P.M. Light fog. Evening, Deposition—light fog.
☉ 7	30.304	30.298	40.3	30.306	30.300	40.0	34	02.2	36.7	36.2	35.0	40.4		N	A.M. Light fog and wind. P.M. Overcast. Evening, Fine and clear—sharp frost.
M 8	30.398	30.392	34.9	30.336	30.330	34.8	25	Water frozen.	30.6	31.5	28.8	37.3		NE	A.M. Light snow, with sharp frost. P.M. Overcast—light wind. Evening, Continued frost.
T 9	30.212	30.204	29.8	30.162	30.156	29.3	23		24.0	23.4	23.0	32.8		N	Overcast—light snow nearly the whole day, with light hrisk wind.
○ W 10	30.050	30.044	28.8	29.972	29.966	29.7	23		25.3	26.5	23.2	25.6		N	A.M. Clondy—light wind, with sharp frost—light snow. P.M. Overcast. Evening, Snow—sharp frost.
T 11	29.960	29.954	27.3	30.002	29.996	28.4	21		22.4	28.0	19.8	23.2		NW	Overcast—light snow and wind, with sharp frost the whole of the day.
F 12	30.324	30.316	28.7	30.334	30.326	28.6	20		23.3	26.9	19.7	24.0		NNW	A.M. Dense fog—light wind. P.M. Overcast—fog cleared. Evening, Fog—sharp frost.
S 13	30.314	30.306	26.9	30.234	30.228	27.6	21		21.8	25.2	20.8	27.7		SE	Fine—light clouds and wind, with sharp frost throughout the day. Evening, Overcast.
☉ 14	29.988	29.984	27.2	29.916	29.912	28.0	20		24.4	25.2	22.0	26.2		NW	A.M. Overcast—light snow—brisk wind. P.M. Lightly overcast. Evening, Fine—sharp frost.
M 15	29.824	29.820	23.6	29.778	29.772	24.3	12		15.7	24.8	14.9	26.3		SW	Overcast—very sharp frost the whole of the day.
T 16	29.850	29.846	25.8	29.922	29.916	27.2	20		27.0	29.3	11.4	26.3		NW	Overcast—light snow, with sharp frost throughout the day. Evening, Very sharp frost.
W 17	30.216	30.208	27.0	30.204	30.198	28.8	21		29.0	30.7	24.2	32.0		NW	A.M. Overcast—light snow and wind. P.M. Fine—light clouds and wind. Evening, Overcast—light snow—frost.
T 18	30.016	30.008	26.9	29.910	29.904	26.3	19		23.3	24.2	23.4	31.5		NE	Overcast—light snow, with light brisk wind throughout the day. Evening, Sharp frost.
F 19	29.786	29.780	25.3	29.786	29.780	25.2	11		21.0	20.5	21.2	25.6		NNE	A.M. Overcast—lt. snow, with light brisk wind. P.M. Fine—lt. clouds. Ev. Overcast.
S 20	29.934	29.928	21.2	29.910	29.904	21.7	14		12.8	21.2	11.5	21.0		NW	A.M. Dense fog. P.M. Fine—lt. clouds. Ev. Light fog—sharp frost.
☉ 21	29.892	29.886	22.4	29.758	29.752	25.0	16		24.5	31.8	13.8	25.0		ENE	A.M. Fine—light clouds—brisk wind. P.M. Cloudy—light brisk wind. Evening, Fine—light clouds—thaw.
M 22	29.680	29.676	27.7	29.700	29.696	31.0	24		39.8	42.8	24.7	40.3		ESE	A.M. Overcast—light wind and rapid thaw. P.M. Fine—lt. clouds—continued thaw. Evening, Overcast.
T 23	29.706	29.700	33.3	29.662	29.658	34.9	33	01.6	34.4	36.4	32.7	43.3		NE	A.M. Overcast—lighbt brisk wind—continned thaw. P.M. Fine—light clouds. Evening, Overcast.
W 24	29.752	29.748	30.9	29.738	29.734	29.6	23	Water frozen.	26.7	25.7	26.4	37.4		NE var.	Overcast—very high wind—sharp frost the whole day. Ev. Sharp frost.
T 25	29.554	29.548	27.6	29.460	29.454	28.6	21		26.7	27.9	24.8	27.0		ENE	Overcast—brisk wind the whole day. Evening, Sharp frost.
● F 26	29.362	29.356	29.0	29.332	29.326	30.0	24		28.6	29.5	26.4	29.2		ENE	Ditto ditto ditto.
S 27	29.284	29.278	30.2	29.280	29.276	30.8	23		29.8	29.8	28.4	30.4		ENE	Overcast—lt. hrisk wind the whole day. Evening, Overcast—frost.
☉ 28	29.454	29.450	31.7	29.452	29.448	32.0	24		28.7	31.0	28.5	31.0		E	Overcast—light fog and wind nearly the whole of the day. Evening, Overcast—sleet and rain.
M 29	29.464	29.458	33.0	29.510	29.502	35.2	29	01.5	37.8	43.8	28.8	38.4		E	A.M. Overcast—rapid thaw—light fog. P.M. Fine—light clouds—continued thaw. Evening, Overcast.
T 30	29.640	29.634	37.2	29.648	29.640	37.2	32	00.8	35.8	34.2	35.8	44.3	.072*	E	Overcast—light fog, with deposition the whole of the day. Evening, Overcast—deposition.
W 31	29.784	29.778	37.3	29.856	29.850	37.2	31	01.3	35.2	35.3	33.8	36.8	* Melted snow.	NE	Overcast—light wind throughout the day. Evening, Overcast.
MEAN.	29.897	29.891	32.5	29.874	29.868	33.1	25.9	01.6	30.1	32.4	27.1	34.4	Sum. .072		Mean Barometer corrected............ 9 A.M. 3 P.M. F. 29.890 .. 29.866 C. 29.883 .. 29.859
FEBRUARY T 1	30.076	30.070	35.7	30.098	30.092	35.4	29	01.5	32.8	34.0	32.4	36.0		N	Overcast—light brisk wind with slight frost throughout the day. Evening, Sharp frost.
F 2	30.324	30.316	35.7	30.344	30.336	36.0	28	00.2	32.3	33.7	31.3	34.6		NNW	A.M. Overcast—lt. wind. P.M. Fine—lt. clds. & wind. Ev. Fine & clear.
S 3	30.368	30.360	33.7	30.372	30.364	34.8	27	02.5	30.8	33.5	29.5	34.4		NNE	A.M. Fine—light clouds and wind. P.M. Overcast—light snow and wind. Evening, Fine and clear.
☉ 4	30.358	30.350	32.0	30.318	30.312	32.2	21	Frozen.	27.3	32.9	26.5	34.4		NNW	Fine—light clouds—sharp frost with brisk wind throughout the day. Evening, Fine and clear.
M 5	30.288	30.282	29.8	30.230	30.224	32.3	23		29.7	35.3	26.3	33.2		NE	A.M. Fine—light clouds—sharp frost with brisk wind. P.M. Overcast. Ev. Fine & clear.
T 6	30.040	30.034	32.0	29.872	29.866	32.8	25		29.8	34.4	28.2	35.8		ENE	Fine—light clouds—sharp frost with brisk wind throughout the day. Evening, Hail—high wind.
W 7	29.514	29.508	34.2	29.382	29.374	36.3	29	03.0	39.3	43.6	29.8	40.0	.208	SSE	A.M. Overcast—light thaw—brisk wind. P.M. Cloudy—continued thaw and wind. Evening, Rain—high wind.
T 8	29.112	29.108	40.4	28.964	28.956	42.5	35	02.6	44.2	46.7	38.8	46.0	.033	SW	A.M. Fine—nearly cloudless—light wind. P.M. Overcast—light rain—high wind. Evening, Overcast—continued rain & wind.
○ F 9	28.708	28.702	43.7	28.656	28.652	44.4	39	02.1	43.7	43.6	41.8	48.4	.100	SW var.	Overcast—lt. rain—h. wind the whole day. Ev. Deposition—lt. wind.
S 10	29.166	29.160	39.8	29.096	29.092	39.6	30	01.9	32.5	35.2	31.6	46.2	.038	NNE	Ovrct.—sharp frost with lt. brisk wind throughout the day. Ev. Cldy.
☉ 11	29.356	29.350	36.2	29.422	29.418	36.5	24	02.8	29.3	35.0	27.3	35 5		NW	Fine—light clouds and wind with sharp frost throughout the day. Evening, Fine and clear.
M 12	29.596	29.590	33.6	29.550	29.542	34.2	26	Water Frozen.	28.4	33.6	26.0	35.3		SW	Overcast—light fog with sharp frost throughout the day. Evening, Fine and clear—sharp frost.
T 13	29.534	29.530	31.4	29.536	29.530	32.8	24		26.9	35.0	24.8	34.5		W	A.M. Fine and clear—sharp frost. P.M. Cloudy—sharp frost. Evening, Cloudy—continued frost.
W 14	29.764	29.760	31.3	29.718	29.710	32.8	24		27.9	35.3	26.5	35.2		NW	Fine—lt. clouds & wind throughout the day. Ev. Fine—light clouds.
T 15	29.638	29.632	30.8	29.574	29.568	31.9	25		28.5	30.5	27.0	36.2		NE	Cloudy—light brisk wind with sharp frost throughout the day. Evening, Fine and clear.
F 16	29.536	29.530	31.3	29.596	29.590	32.6	21		29.9	31.8	28.0	31.6		NE	Fine—light clouds—brisk wind with sharp frost nearly the whole of the day. Evening, Heavy snow.
S 17	29.644	29.638	30.8	29.660	29.654	32.0	25		28.6	33.0	26.3	32.5		NE	Overcast—hail, rain, and snow, with high wind. Evening, Thaw—light rain and wind.
☉ 18	30.048	30.042	33.3	30.130	30.126	34.2	28	01.8	33.2	35.0	28.7	35.3	.222	NW	Overcast—thaw with light wind throughout the day.
M 19	30.166	30.158	34.2	30.048	30.042	36.2	29	01.4	33.5	37.8	32.3	35.5		NE	Overcast—light brisk wind with slight thaw. Evening, Fine and clear—sharp frost.
T 20	29.702	29.698	33.7	29.638	29.632	35.8	25	02.2	31.3	39.7	29.0	38.2		E	A.M. Fine—light clouds and wind. P.M. Overcast—light wind. Evening, Very fine rain.
W 21	29.706	29.700	36.4	29.704	29.698	37.6	31	01.4	35.3	37.7	31.0	40.4		NNE	Overcast—deposition—light wind throughout the day. Ev. Overcast.
T 22	29.700	29.694	36.2	29.644	29.640	37.0	30	02.0	34.2	36.9	32.2	38.3		NE	Overcast—light brisk wind the whole of the day.
F 23	29.478	29.472	37.2	29.378	29.372	37.2	32	01.1	33.8	34 8	33.6	37.4		NE var.	A.M. Overcast—deposition—brisk wind. P.M. Light rain and wind. Evening, Continued rain and wind.
● S 24	28.770	28.768	38.6	28.688	28.684	39.9	35	00.6	39.8	44.5	33.5	40.6	.416	ENE	Overcast—light steady rain with brisk wind nearly the whole of the day. Evening, Fine and clear.
☉ 25	28.754	28.750	41.5	28.804	28.800	44.0	36	02.9	43.7	48.5	39.7	46.0	.250	SW	A.M. Cloudy—brisk wind. P.M. Fine—light clouds and wind. Evening, Fine and clear.
M 26	28.950	28.946	41.8	28.974	28.968	41.7	37	01.6	38.8	39.3	38.2	48.7		NE	A.M. Overcast—lt. brisk wind. P.M. Light rain. Ev. Heavy rain.
T 27	29.136	29.134	40.2	29.148	29.144	40.6	35	01.7	37.7	38.3	35.0	40.4	.333	ENE	Overcast—light rain with high wind the whole of the day. Evening, Continued rain.
W 28	29.114	29.108	41.4	29.152	29.148	43.8	36	01.0	42.2	47.8	37.3	43.4	.077	S	A.M. Cloudy—light wind. P.M. Fine—light clouds and wind. Evening, Overcast—light rain and wind.
MEAN.	29.591	29.585	35.6	29.561	29.555	36.7	28.9	01.8	33.8	37.4	31.2	38.4	Sum. 1.677		Mean Barometer corrected .... 9 A.M. 3 P.M. F. 29.577 .. 29.544 C. 29.570 .. 29.536

1838.	9 o'clock, A.M. Barometer uncorrected. Flint Glass.	9 o'clock, A.M. Barometer uncorrected. Crown Glass.	9 o'clock, A.M. Att. Ther.	3 o'clock, P.M. Barometer uncorrected. Flint Glass.	3 o'clock, P.M. Barometer uncorrected. Crown Glass.	3 o'clock, P.M. Att. Ther.	Dew Point at 9 A.M., deg. Fahr.	Diff. of Wet and Dry Bulb Thermometer.	External Thermometers. Fahrenheit. 9 A.M.	External Thermometers. Fahrenheit. 3 P.M.	External Thermometers. Self-registering. Lowest	External Thermometers. Self-registering. Highest	Rain in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
MARCH															
T 1	29.124	29.118	44.3	29.104	29.098	45.6	39	01.8	43.7	47.5	41.8	49.2	.133	S	Lightly overcast, with light wind throughout the day. Evening, Light rain.
F 2	29.094	29.090	45.0	29.122	29.116	45.3	40	01.5	41.7	44.6	40.9	48.0	.088	NE	A.M. Overcast—deposition—light wind. P.M. Overcast—light rain.
S 3	29.246	29.242	44.9	29.222	29.218	45.7	39	02.0	42.0	44.3	41.4	45.0	.063	W	Overcast—light wind throughout the day, as also the evening.
☉ 4	29.030	29.024	42.8	29.000	28.998	43.6	38	00.3	38.5	45.2	37.6	45.3	.105	NE	Overcast—light rain with high wind the whole of the day.
M 5	29.614	29.606	43.7	29.820	29.814	44.8	37	02.1	41.2	43.7	38.4	47.6	.108	NW	Overcast—light wind throughout the day. Evening, Light rain and wind.
T 6	29.796	29.792	46.0	29.826	29.820	47.3	40	02.8	46.6	50.8	40.7	47.0	.161	WSW v.	A.M. Fine—light clouds—brisk wind. P.M. Cloudy—brisk wind. Evening, Fine and clear.
W 7	29.912	29.906	44.8	29.876	29.870	46.4	37	02.1	41.9	49.4	37.0	51.3		W	Fine—light clouds and wind throughout the day.
T 8	30.116	30.108	43.7	30.200	30.194	45.6	36	03.2	42.2	47.3	37.8	50.0		NW	A.M. Cloudy—light brisk wind. P.M. Fine—light clouds and wind. Evening, Fine and clear.
F 9	30.302	30.296	43.2	30.216	30.208	44.2	37	02.0	37.3	44.2	35.2	47.8		NW	A.M. Thick fog. P.M. Fine—light clouds and wind. Evening, Fine—light clouds.
S 10	30.000	29.996	42.0	29.922	29.916	44.4	38	02.9	40.3	45.3	35.0	44.9		SE	Fine—light clouds and wind throughout the day.
○☉ 11	29.778	29.772	43.2	29.780	29.776	44.0	36	03.1	39.8	45.5	36.0	46.4		E	Fine—nearly cloudless—light wind the whole of the day. Evening, Fine and clear.
M 12	30.120	30.114	41.8	30.162	30.154	43.3	35	02.2	38.8	46.4	35.9	46.0		N	A.M. Light fog. P.M. Fine and cloudless. Evening, Fine & clear.
T 13	30.232	30.226	42.2	30.140	30.132	44.2	37	02.8	41.8	48.2	37.0	46.8		SE	A.M. Overcast—light wind. P.M. Light rain. Evening, Overcast.
W 14	30.062	30.054	46.3	30.006	30.000	49.2	43	03.0	50.6	58.0	41.8	51.4		W	Lightly overcast, with light wind throughout the day. Ev. Cloudy.
T 15	29.948	29.944	49.3	29.976	29.970	49.7	43	03.1	46.5	47.3	44.6	58.3		NW	Cloudy—light wind the whole of the day. Evening, Fine & clear.
F 16	29.900	29.896	48.2	29.692	29.686	49.6	39	03.0	41.8	48.6	37.2	48.4		W	A.M. Fine—nearly cloudless—light wind. P.M. Fine—light clouds—brisk wind. Evening, Overcast—heavy rain.
S 17	29.480	29.476	47.0	29.436	29.430	46.8	36	04.8	41.3	45.8	36.3	50.0	.180	NW	Fine—lt. clouds—brisk wind throughout the day. Ev. Fine & clear.
☉ 18	29.618	29.612	46.2	29.618	29.614	45.0	36	04.0	41.0	45.0	36.2	47.0		W	A.M. Fine—lt. clouds & wind. P.M. Cloudy—lt. wind. Ev. Overcast.
M 19	29.610	29.602	44.0	29.618	29.612	45.4	38	02.8	41.2	46.4	39.8	46.0		S	Overcast—light mist nearly the whole of the day. Evening, Overcast—high wind.
T 20	29.222	29.216	47.8	29.256	29.250	49.3	42	03.7	50.2	49.4	41.0	51.7		SW var.	Fine—light clouds—high wind, with occasional hail and rain during the day. Evening, Fine and clear.
W 21	29.394	29.390	50.4	29.338	29.334	46.7	38	05.3	43.7	48.3	36.0	53.3	.055	SSW	A.M. Fine—light clouds and wind. P.M. Cloudy—high wind. Evening, Fine and clear.
T 22	29.522	29.516	45.0	29.564	29.560	45.3	37	03.2	40.0	40.4	35.8	49.3		NW	Overcast—brisk wind throughout the day. Evening, Fine & clear.
F 23	29.580	29.576	44.9	29.518	29.512	43.2	31	03.9	36.3	38.8	31.0	41.8		NW	A.M. Fine—light clouds—brisk wind. P.M. Overcast—light snow, as also in the evening.
S 24	29.566	29.560	43.3	29.522	29.516	44.2	35	03.8	41.0	43.5	35.4	42.6		SSW	A.M. Fine—light clouds and wind. P.M. Overcast—brisk wind. Evening, Light rain.
●☉ 25	29.820	29.814	49.0	29.932	29.928	45.5	37	05.1	44.8	49.3	36.4	47.2	.036	WSW	Fine—light clouds and wind the whole of the day.
M 26	30.130	30.124	42.9	30.122	30.116	45.8	35	04.0	42.7	51.3	35.2	50.8		NW	Overcast—light brisk wind the whole of the day.
T 27	30.312	30.304	45.0	30.310	30.302	46.2	38	03.5	43.9	53.3	38.4	51.4		NE	A.M. Overcast—light fog and wind. P.M. Fine—light clouds and wind. Evening, Fine and clear.
W 28	30.524	30.518	48.8	30.508	30.500	48.3	40	00.4	43.8	52.0	40.0	54.4		N	A.M. Fine and cloudless—light wind. P.M. Fine and cloudless—light haze. Evening, Fine and clear.
T 29	30.530	30.524	46.0	30.506	30.500	48.3	40	01.8	40.3	56.8	38.7	52.5		NE	A.M. Thick fog. P.M. Fine—light clouds and wind. Ev. Overcast.
F 30	30.488	30.480	50.5	30.370	30.364	51.7	46	04.9	51.8	55.8	40.2	57.0		NNW	A.M. Overcast—light wind. P.M. Fine—light clouds and haze. Evening, Fine and starlight.
S 31	30.180	30.172	49.8	30.134	30.128	50.7	43	03.1	45.8	43.8	41.8	56.4		NNW	A.M. Thick haze. P.M. Overcast. Evening, Cloudy.
MEAN.	29.815	29.809	46.2	29.801	29.795	46.3	38.2	02.9	42.7	47.6	38.1	49.2	Sum. .929		Mean Barometer corrected .... 9 A.M. F. 29.772, C. 29.765 .. 3 P.M. F. 29.758, C. 29.751
APRIL															
☉ 1	30.204	30.198	51.4	30.164	30.158	44.5	32	02.2	34.5	40.0	30.3	51.6		NNE	A.M. Fine—light clouds and wind—sharp frost during the night. P.M. Overcast. Evening, Fine and clear—sharp frost.
M 2	30.056	30.050	46.2	29.914	29.906	43.3	29	01.3	36.3	46.0	29.8	40.8		WSW	Fine—light clouds & wind throughout the day. Ev. Fine—light clouds.
T 3	29.978	29.972	48.5	29.934	29.926	43.7	33	01.6	39.4	43.4	34.3	47.3		N	A.M. Fine—light clouds and wind—sharp frost during the night. P.M. Overcast—light wind. Evening, Cloudy.
W 4	29.928	29.922	47.2	29.942	29.936	47.0	39	03.9	47.8	54.4	39.3	49.7	.016	W	A.M. Fine—light clouds and wind—rain during the night. P.M. Overcast. Evening, Dark heavy clouds.
T 5	30.014	30.008	52.2	29.934	29.926	50.3	40	02.4	51.4	56.2	44.9	55.2		W	A.M. Fine—nearly cloudless—lt. wind. P.M. Cloudy—lt. wind. Ev. [The same.
F 6	29.706	29.700	50.6	29.726	29.720	53.2	45	03.5	52.8	58.3	48.3	56.9	.036	W	A.M. Overcast—light wind—rain during the night—overcast the rest of the day. Rain at night.
S 7	29.550	29.542	52.5	29.326	29.320	53.4	48	03.6	51.7	52.6	50.3	58.8	.033	SW	Overcast—high wind throughout the day, with occasional rain.
☉ 8	29.188	29.182	49.7	29.340	29.334	50.6	42	02.9	41.8	48.9	41.5	52.8	.200	W	A.M. Overcast—light rain with high wind. Fine—light clouds with high wind the rest of the day.
M 9	29.762	29.756	50.7	29.868	29.862	50.3	37	04.9	43.7	47.6	40.0	49.6		NW	Fine—lt. clouds with brisk wind throughout the day. Ev. Fine & clear.
○ T 10	30.060	30.052	51.2	30.064	30.056	51.6	42	05.8	50.7	58.0	41.2	51.5		SSW	A.M. Cloudy—brisk wind. P.M. Fine—light clouds—high wind. Evening, Fine—light clouds.
W 11	30.184	30.178	60.8	30.084	30.078	54.7	46	00.6	54.0	63.4	48.8	59.2		S	A.M. Fine—light clouds & wind. P.M. Fine & cloudless. Ev. Cloudy.
T 12	30.230	30.222	60.2	30.194	30.188	54.6	44	04.9	49.6	55.7	43.0	64.2		NW	Fine and cloudless—light wind throughout the day. Ev. Fine & clear.
F 13	30.276	30.270	58.3	30.180	30.172	52.3	40	02.6	47.0	48.5	38.0	56.4		NW	A.M. Fine—light clouds and wind. Overcast the rest of the day.
S 14	29.986	29.980	49.4	30.002	29.996	49.7	42	04.9	46.2	49.7	43.8	50.8		S	Overcast throughout the day.
☉ 15	29.792	29.786	49.0	29.692	29.688	51.8	42	02.6	47.0	55.8	44.2	50.3		SW	Overcast—lt. hrisk wind throughout the day. Ev. Light rain—h. wind.
M 16	29.718	29.712	51.7	29.664	29.658	49.2	34	06.6	43.5	43.4	37.8	56.8	.050	NW var.	Overcast—high wind throughout the day, with occasional rain & snow.
T 17	29.628	29.620	49.3	29.648	29.642	46.2	32	05.0	40.8	45.3	33.3	46.7	.025	NW	A.M. Lightly overcast—high wind. P.M. Light rain. Evening, Overcast—hail and rain, with high wind.
W 18	29.724	29.716	44.2	29.716	29.708	44.6	33	04.6	38.7	44.0	34.4	46.3	.075	NW	Overcast—light hrisk wind throughout the day. Evening, Snow and rain with high wind.
T 19	29.716	29.708	41.8	29.706	29.698	43.7	35	02.5	36.2	42.8	34.4	44.7	.011	NW	Overcast—lt. wind with occasional rain throughout the day. Ev. Hail.
F 20	29.772	29.766	44.9	29.720	29.712	43.5	34	03.4	41.3	38.3	35.3	44.0		NNW	A.M. Fine—light clouds. P.M. Overcast—light snow, rain, and wind. Evening, Light rain.
S 21	29.658	29.652	42.3	29.556	29.550	44.7	35	04.5	41.8	47.4	36.2	44.0	.127	NW	A.M. Overcast—light wind. P.M. Fine—light clouds and wind. Evening, Fine and clear—high wind.
☉ 22	29.182	29.176	42.7	29.250	29.246	44.2	35	04.4	41.9	45.5	36.8	48.3		E var.	A.M. Overcast—very light rain—high wind. High wind throughout the night. P.M. Overcast.
M 23	29.372	29.366	49.3	29.296	29.290	48.8	37	04.6	50.3	52.8	41.0	51.5		S	A.M. Cloudy—lt. wind. P.M. Fine—lt. clouds & wind. Ev. Overcast [—lt. rain.
● T 24	29.460	29.454	47.4	29.566	29.558	48.8	40	03.5	44.8	49.4	40.8	54.7	.055	NE	A.M. Overcast—brisk wind. P.M. Fine—light clouds and wind. Evening, Light rain.
W 25	29.740	29.732	51.4	29.756	29.750	49.7	41	03.5	47.0	46.4	38.2	50.6	.036	NNE	Lightly overcast—brisk wind nearly the whole of the day. Evening, Lightning, with high wind.
T 26	29.864	29.856	47.6	29.904	29.898	48.9	42	02.6	46.0	44.9	43.0	51.0		NW	Overcast—high wind throughout the day.
F 27	29.926	29.920	47.8	29.900	29.892	47.8	37	05.0	43.8	46.8	37.8	49.2		NNE	A.M. Lightly overcast—high wind. P.M. Fine—light clouds and wind. Evening, Fine and clear—high wind.
S 28	29.840	29.836	51.3	29.764	29.756	46.8	35	05.5	43.8	48.7	35.0	47.7		NNE	A.M. Fine—light clouds—high wind. High wind throughout the night. P.M. Fine—nearly cloudless. Ev. Overcast.
☉ 29	29.708	29.704	60.3	29.696	29.692	48.0	32	06.7	41.2	46.2	36.2	50.4		NW	A.M. Fine—lt. clds. & wind. P.M. Ovct.—lt. snow & wind. Ev. Very lt. [rain.
M 30	29.546	29.540	45.7	29.506	29.500	48.2	35	05.4	45.7	50.5	38.8	47.0		SE	A.M. Overcast—light brisk wind. P.M. Very light rain. Evening, Rainbow visible, with lt. rain—rest of evening overcast—lt. rain.
MEAN.	29.792	29.786	49.9	29.767	29.761	48.5	37.9	03.8	44.7	49.0	39.2	50.9	Sum. .664		Mean Barometer corrected .... 9 A.M. F. 29.739, C. 29.732 .. 3 P.M. F. 29.718, C. 29.711

1838.	9 o'clock, A.M. Barometer uncorrected. Flint Glass.	9 o'clock, A.M. Barometer uncorrected. Crown Glass.	9 o'clock, A.M. Att. Ther.	3 o'clock, P.M. Barometer uncorrected. Flint Glass.	3 o'clock, P.M. Barometer uncorrected. Crown Glass.	3 o'clock, P.M. Att. Ther.	Dew Point at 9 A.M., deg. Fahr.	Diff. of Wet and Dry Bulb Thermometer.	External Thermometers. Fahrenheit. 9 A.M.	External Thermometers. Fahrenheit. 3 P.M.	External Thermometers. Self-registering. Lowest	External Thermometers. Self-registering. Highest	Rain in inches. Read off at 9 A.M.	Direction of the Wind at 9 A.M.	REMARKS.
MAY															
T 1	29.632	29.626	48.4	29.598	29.592	50.3	42	04.0	48.8	53.6	45.3	53.0	.047	SSE	Overcast—light wind throughout the day, with occasional rain.
W 2	29.840	29.834	65.6	29.800	29.796	57.4	48	06.4	59.3	63.7	48.3	61.3	.111	S	A.M. Cloudy—light brisk wind. P.M. Fine—light clouds. Evening, Fine—light clouds. Distant thunder with vivid lightning.
T 3	29.870	29.864	56.6	29.838	29.832	58.4	50	05.1	56.7	61.2	51.7	64.6		S	A.M. Cloudy—light wind. P.M. Fine—light clouds—brisk wind. Evening, Fine and clear.
F 4	30.014	30.006	62.9	29.964	29.956	60.4	52	06.4	58.4	65.4	48.8	63.0		NE	A.M. Cloudy—lt. wind. P.M. Fine—lt. clouds. Evening, Fine & clear.
S 5	30.136	30.132	61.8	30.126	30.122	59.9	50	08.0	58.9	60.7	49.4	65.6		NW	A.M. Fine—light clouds—brisk wind. P.M. Cloudy—rain, with brisk wind. Evening, Fine and clear.
☉ 6	30.308	30.296	54.4	30.268	30.264	57.0	47	03 6	47.4	61.0	45.8	64.4	.105	NE	A.M. Lightly overcast—light wind. P.M. Fine—light clouds and wind. Evening, Fine and clear.
M 7	30.328	30.324	65.4	30.272	30.264	59.2	42	07.0	54.4	72.8	42.3	61.4		N	Fine & cloudless—light wind throughout the day. Ev. Fine & clear.
T 8	30.328	30.322	64.2	30.262	30.254	61.7	51	08.9	64.2	73.4	50.3	73.2		NE	Fine & cloudless—light wind throughout the day. Ev. Fine & clear.
○ W 9	30.200	30.198	75.0	30.122	30.116	62.4	50	10.7	59.9	68.3	48.8	73.3		N	A.M. Fine—very light clouds and wind. P.M. Fine and cloudless. Evening, Lightly overcast.
T 10	30.346	30.340	63.6	30.370	30.362	57.3	42	09.0	51.3	52.7	43.8	70.3		NE var.	A.M. Fine—light clouds and wind. P.M. Fine—nearly cloudless—brisk wind. Evening, Fine and clear.
F 11	30.394	30.390	65.6	30.294	30.288	55.8	38	07.9	49.4	57.0	38.3	55.3		NE	A.M. Light clouds and wind. P.M. Fine and cloudless. Evening, Fine and clear.
S 12	30.148	30.144	62.4	30.002	29.994	56.6	41	08.9	53.4	63.8	41.7	58.4		N	Fine & cloudless—lt. wind throughout the day. Ev. Lightly overcast.
☉ 13	29.686	29.678	54.9	29.638	29.634	57.0	44	04.5	49.7	49.6	46.2	63.6		NW	Overcast—light wind, with occasional light rain througbout the day. Evening, Cloudy.
M 14	29.668	29.660	56.4	29.654	29.646	54.3	38	05.2	46.8	49.4	40.4	56.2		NW	Cloudy—light wind throughout the day. Evening, Fine and clear.
T 15	29.764	29.758	62.8	29.732	29.728	52.8	35	06.6	45.2	50.8	36.2	53.6		N	Fine—light clouds and wind throughout the day. Ev. Fine & clear.
W 16	29.858	29.850	61.2	29.818	29.810	52.3	32	06.7	47.4	51.7	36.8	54.7		E	Fine—light clouds and wind throughout the day. Ev. Fine & clear.
T 17	29.812	29.804	50.3	29.770	29.764	51.4	37	05.7	47.3	55.0	38.9	52.8		NE	A.M. Lightly overcast—light wind. P.M. Fine—light clouds and wind. Evening, Cloudy.
F 18	29.866	29.860	64.3	29.886	29.878	53.6	38	09.4	49.3	54.3	39.0	55.4		NW	Fine—nearly cloudless—light wind throughout the day. Evening, Fine and clear.
S 19	29.826	29.818	53.0	29.814	29.806	53.3	40	06.6	50.0	57.7	40.9	55.2		E	Overcast—light brisk wind throughout the day. Evening, Cloudy.
☉ 20	29.592	29.586	53.2	29.536	29.532	54.8	45	05.1	52.5	56.0	49.6	58.6		E	A.M. Overcast—brisk wind. P.M. Overcast—light rain. Evening, Continued rain.
M 21	29.540	29.532	61.7	29.576	29.570	58.3	46	07.1	56.7	60.4	50.2	65.0	.038	S	A.M. Overcast—light hrisk wind. P.M. Fine—light clouds—high wind. Evening, Overcast.
T 22	29.530	29.522	57.5	29.592	29.586	57.8	50	05.4	54.5	55.3	52.0	60.7		SE	A.M. Overcast—very lt. rain. P.M. Overcast—lt. wind. Ev. Cloudy.
● W 23	29.796	29.788	58.8	29.810	29.802	56.9	45	08.0	55.0	56.7	46.8	59.3	.033	NW	A M. Cloudy—lt. brisk wind. P.M. Overcast. Ev. Overcast—lt. rain.
T 24	29.900	29.896	55.0	29.918	29.912	56.9	46	04.2	51.7	56.8	48.3	57.4	.041	NW	Overcast—light wind throughout the day. Evening, Fine & clear. Rain during the night.
F 25	30.012	30.004	55.7	29.982	29.976	57.3	47	02.7	49.7	61.2	46.8	57.7		N	A.M. Overcast—lt. wind. P.M. Fine—lt. clouds & wind. Ev. Overcast.
S 26	30.076	30.068	53.3	30.084	30.078	55.8	47	04.3	50.8	55.3	46.2	61.2		NE	A.M. Overcast—light brisk wind. P.M. Fine—nearly cloudless. Evening, Fine—light clouds.
☉ 27	29.978	29.970	58.8	29.864	29.860	55.5	44	06.1	51.4	56.5	42.6	59.8		NE	A.M. Overcast—light brisk wind. P.M. Fine—light clouds—brisk wind. Evening, Fine and starlight.
M 28	29.560	29.554	54.2	29.574	29.568	56.9	50	00.7	53.4	59.0	48.2	56.7	.077	ENE	A.M. Overcast—light rain. P.M. Overcast—light wind. Evening, Fine and clear. Rain during the night.
T 29	29.628	29.624	67.7	29.670	29.664	61.2	52	07.7	60.2	64.8	51.6	69.7	.166	SSE	A.M. Cldy—lt. wind. P.M. Fine—lt. clds.—h. wind. Ev. Fine & clear.
W 30	29.808	29.800	62.2	29.838	29.832	61.8	51	06.5	59.3	66.4	52.8	70.4	.013	S	A.M. Lightly overcast. P.M. Fine—light clouds and wind. Evening, Fine and clear.
T 31	29.924	29.920	67.8	29.928	29.920	63.8	53	07.5	62.2	68.2	52.2	67.0		S	A.M. Fine—light clouds and wind. P.M. Lightly cloudy. Evening, Thunder storm, with lightning and heavy rain.
MEAN.	29.915	29.909	59.8	29.890	29.884	57.0	44.9	09.2	53.4	59.3	45.8	61.3	Sum. .571		Mean Barometer corrected .... 9 A.M. F. 29.835 .. C. 29.827; 3 P.M. F. 29.818 .. C. 29.811
JUNE															
F 1	29.974	29.970	66.0	29.920	29.912	63.7	56	07.8	60.8	61.3	52.7	70.3	.372	NW	A.M. Fine—light clouds. P.M. Overcast—heavy rain. Evening, Continued rain.
S 2	29.836	29.830	62.6	29.780	29.772	64.2	55	06.4	59.3	65.7	52.5	70.3	.644	S	A.M. Lightly overcast—lt. wind. P.M. Heavy rain. Ev. Fine & clear.
☉ 3	29.800	29.796	64.7	29.754	29.750	64.0	55	07.4	59.5	63.9	51.3	66.0	.341	S	A.M. Cloudy—heavy rain—lt. wind. P.M. Overcast. Ev. Cloudy.
M 4	29.732	29.728	67.0	29.740	29.734	63.7	54	08.1	61.5	65.3	53.6	66.6	.055	S	Fine—light clouds and wind, with occasional showers. Evening, Fine and clear.
T 5	29.924	29.920	67.8	29.906	29.900	64.2	54	07.2	58.6	67.8	50.4	70.5	.022	SSW	Cloudy throughout the day.
W 6	30.052	30.044	59.8	30.100	30.096	60.4	52	03.8	52.7	55.5	51.0	69.3		NW	Overcast—light wind throughout the day. Ev. Fine and clear.
T 7	30.212	30.204	60.6	30.162	30.154	61.0	51	06.1	54.9	62.5	50.6	58.7		NW	Cloudy—light wind throughout the day. Evening, Fine and clear.
○ F 8	30.304	30.298	79.3	30.278	30.270	61.6	42	09.1	51.7	57.3	44.2	64.3		NW	Fine—light clouds and wind throughout the day. Ev. Fine & clear.
S 9	30.240	30.234	64.6	30.128	30.120	61.8	48	08.0	59.0	63.4	47.0	70.3		SW	A.M. Fine—light clouds and wind. P.M. Cloudy—light wind. Evening, Cloudy.
☉ 10	29.798	29.792	58.9	29.682	29.678	61.0	50	07.8	59.3	62.0	50.5	64.2		S	A.M. Cloudy—light brisk wind. P.M. Overcast—light wind. Evening, Heavy rain.
M 11	29.518	29.512	58.6	29 [illegible]38	29.532	61.0	53	03.7	55.2	54.7	52.3	63.0	.227	NE	Overcast—light wind, with heavy showers the whole of the day.
T 12	29.564	29.556	60.0	2[illegible] 00	29.594	61.3	54	05.5	55.7	60.0	52.4	60.8	.533	S	A.M. Dark heavy clouds—rain early. P.M. Heavy rain—hail and thunder. Evening, Fine & clear.
W 13	29.682	29.676	59.3	[illegible]680	29.674	60.6	53	05.3	56.8	61.3	50.8	62.8	.366	NW	A.M. Overcast—light wind. P.M. Fine—light clouds and wind. Evening, Overcast—heavy rain.
T 14	29.744	29.736	60.7	29.780	29.774	63.0	54	06.8	59.8	63.4	51.2	63.0	.100	S	A.M. Fine—lt. clouds & wind. P.M. Cloudy—brisk wind. Ev. Lt. rain.
F 15	29.784	29.776	63.6	29.728	29.722	62.8	55	06.1	60 7	61.4	54.0	64.5	.025	S	Overcast—light wind throughout the day, with occasional light rain.
S 16	29.800	29.792	61.2	29.836	29.828	64.3	54	06.2	61.8	69.3	54.8	65.6	.013	S	Lightly overcast—light wind throughout the day. Ev. Light rain.
☉ 17	29.888	29.882	64.5	29.858	29.854	66.6	60	05.4	63.5	70.6	60.3	70.4	.172	S	Overcast—light wind the whole of the day. Rain during the night.
M 18	29.682	29.678	68.9	29.576	29.572	69.2	62	06.2	68.3	69.8	61.0	71.5	.022	ENE	A.M. Overcast—brisk wind—thunder and lightning, with heavy rain. P.M. Dark clouds—high wind. Evening, Light rain.
T 19	29.826	29.820	70.8	29.872	29.866	67.3	58	08.2	63.8	66.4	54.7	71.7	.177	S	A.M. Fine—lt. clouds & wind. P.M. Cloudy—lt. wind. Ev. Overcast.
W 20	29.736	29.728	63.8	29.708	29.704	65.2	60	04.9	61.0	64.4	56.2	67.3	.283	SW	Overcast—light rain and wind throughout the day.
T 21	29.590	29.584	63.9	29.692	29.686	65.6	58	05.2	60.8	63.8	59.0	67.2	.033	W	A.M. Lightly overcast—brisk wind. P.M. Overcast—heavy shower. Evening, High wind.
● F 22	29.860	29.854	66.6	29.880	29.872	65.0	57	07.1	62.4	64.8	53.6	72.3		SW var.	A.M. Dark heavy clouds—brisk wind. P.M. Fine—light clouds and wind. Evening, Fine and clear.
S 23	30.192	30.186	74.6	30.148	30.142	66.3	56	07.3	62.4	70.4	51.7	72.6	.227	NW	Fine—very light clouds the whole of the day. Ev. Fine and clear.
☉ 24	30.052	30.048	75.5	29.996	29.992	68.0	59	09.3	66.7	76.5	54.8	79.7		E	Fine—nearly cloudless—lt. wind throughout the day. Ev. Fine & clear.
M 25	30.090	30.082	70.0	30.072	30.066	70.4	61	08.1	68.3	71.8	61.6	77.3		NNW	Cloudy—light wind the whole of the day. Ev. Overcast—light rain.
T 26	30.000	29.994	65.3	29.992	29.984	66.3	60	03.1	57.4	61.0	56.0	72.8	.572	N	A.M. Overcast—light rain—heavy rain during the night. P.M. Overcast. Evening, Very light rain.
W 27	30.044	30.036	69.2	30.038	30.030	66.8	59	07.9	65.0	61.8	57.0	74.6	.019	SW	A.M. Cloudy—very light rain and wind. P.M. Overcast—light wind.
T 28	30.050	30.044	65.0	30.014	30.006	66.8	58	06.6	62.9	65.6	57.3	66.3		S	Overcast—light wind throughout the day.
F 29	29.998	29.992	75.7	30.004	29.996	67.2	57	09.0	62.8	59.0	54.3	72.8		W	A.M. Fine—light clouds and wind. P.M. Overcast—thunder—light rain. Evening, Light rain.
S 30	29.974	29.970	74.8	29.914	29.906	66.0	56	09.1	62.8	66.3	52.0	72.7	.169	S	Fine—light clouds and wind throughout the day. Evening, Overcast—light rain.
MEAN.	29.898	29.892	66.1	29.879	29.873	64.5	55.4	06.8	60.5	64.2	53.6	68.6	Sum. 4.372		Mean Barometer corrected .... 9 A.M. F 29.802 .. C. 29.795; 3 P.M. F 29.787 .. C. 29.780